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Vol. 74 September 1980 No. 3

TABLE OF CONTENTS

Candidates and Parties in Congressional Elections .... Thomas E. Mann & Raymond E. Wolfinger 617

A Comparison of Voting for U.S. Senator and Representative in 1978 .... Alan I. Abramowitz 633

The American Voter in Congressional Elections .... Barbara Hinckley 641

A Dynamic Model of Presidential Nomination Campaigns .... John H. Aldrich 651

Self-Interest vs. Symbolic Politics in Policy Attitudes and Presidential Voting .... David O. Sears et al. 670

The Levels of Conceptualization: False Measures of Ideological Sophistication .... Eric R. A. N. Smith 685

The Effects of Charges of Corruption on Voting Behavior in Congressional Elections .... John G. Peters & Susan Welch 697

The Rationality of Hegel's Concept of Monarchy .... Bernard Yack 709

Motive and Goal in Hannah Arendt's Concept of Political Action .... James T Knauer 721

Creating the Statutory State: The Implications of a Rule of Law Standard in American Politics .... Richard F. Bensel 734

Can Government Regulate Safety? The Coal Mine Example .... Michael S. Lewis-Beck & John R. Alford 745

The Emergence of Political Parties in Congress, 1789–1803 .... John F. Hoadley 757

Communications 780

Editorial Note 793

Book Reviews

- Political Theory and Methodology 795
- American Politics 812
- Comparative Politics 835
- International Politics 873
- Reference Sources 894

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BOOK REVIEWS

Political Theory and Methodology

Beitz, Charles R. *Political Theory and International Relations* ... Terry Nardin 795
Eibl-Eibesfeldt, Irenäus, trans. by Eric Mosbacher. *The Biology of Peace and War: Men, Animals, and Aggression* ... James C. Davies 796
Falco, Maria J., ed. *Through the Looking-glass: Epistemology and the Conduct of Inquiry–An Anthology* ... Eugene F. Miller 797
Fishkin, James S. *Tyranny and Legitimacy: A Critique of Political Theories* ... Paul F. Kress 801
Johnston, R. J. *Political, Electoral, and Spatial Systems: An Essay in Political Geography* ... John C. Blydenburgh 799
Kolakowsky, Leszek; trans. by P. S. Falla. *Main Currents of Marxism: Its Rise, Growth, and Dissolution:* Vol. 1, *The Founders*; Vol. 2, *The Golden Age;* Vol. 3, *The Breakdown* ... Theodore M. Norton 799
Laslett, Peter, and James S. Fishkin, eds. *Philosophy, Politics and Society* ... Paul F. Kress 801
Lindblom, Charles E., and David K. Cohen. *Usable Knowledge: Social Science and Social Problem Solving* ... Robert F. Rich 803
Marvick, Elizabeth W., ed. *Psychopolitical Analysis: Selected Writings of Nathan Leites* ... Fred I. Greenstein 804
Metaxas, Anastase-Jean. *Systémismes et Politique* ... Albert A. Mavrinac 805
Mitroff, Ian I., and Ralph H. Kilmann. *Methodological Approaches to Social Sciences* ... Henry Teune 805
Okin, Susan M. *Women in Western Political Thought* ... Alan Ritter 806
Pennock, J. Roland, and John W. Chapman, eds. *Compromise in Ethics, Law, and Politics: Nomos XXI* ... Donald A. Strickland 807
Rousseau, Jean-Jacques; trans. by Charles E. Butterworth. *The Reveries of the Solitary Walker* ... Mary L. Bellhouse 808
Ryan, Alan, ed. *The Idea of Freedom: Essays in Honour of Isaiah Berlin* ... Robert A. Kocis 810
Wilhoit, Francis M. *Quest for Equality in Freedom* ... Klaus H. Heberle 811

American Politics

Alexander, Herbert E. *Financing the 1976 Election* ... Michael Margolis 812
Blair, John P., and David Nachmias, eds. *Fiscal Retrenchment and Urban Policy* ... Timothy G. O'Rourke 813
Brams, Steven J. *The Presidential Election Game* ... Bert A. Rockman 814
Brown, Seyom. *The Crises of Power: An Interpretation of United States Foreign Policy During the Kissinger Years* ... Roy M. Melbourne 816
Ceaser, James W. *Presidential Selection: Theory and Development* ... William Crotty 817
Clark, Duncan L. *Politics of Arms Control: The Role and Effectiveness of the U.S. Arms Control and Disarmament Agency* ... John H. Esterline 818
Cunningham, Noble, Jr., ed. *Circular Letters of Congressmen to their Constituents, 1789–1829,* 3 vols. ... Samuel Kernell 819
Feeley, Malcolm M. *The Process is the Punishment: Handling Cases in a Lower Criminal Court* ... Burton M. Atkins 820
Franck, Thomas M., and Edward Weisband. *Foreign Policy By Congress* ... Christopher J. Deering 822
Hanna, Mary T. *Catholics and American Politics* ... John Fenton 823
Hollingsworth, J. Rogers, and Ellen Jane Hollingsworth. *Dimensions in Urban History: Historical and Social Science Perspectives on Middle-Size American Cities* ... Harvey Boulay 824
Houseman, Gerald L. *The Right of Mobility* ... William C. Louthan 825
Jones, Rochelle, and Peter Woll. *The Private World of Congress* ... Sidney Wise 826
Kyvig, David E. *Repealing National Prohibition* ... Harry R. Mahood 828
Livingston, John C. *Fair Game? Inequality and Affirmative Action* ... Richard A. Brisbin, Jr. 828
Martin, John F. *Civil Rights and the Crisis of Liberalism: The Democratic Party 1945–1976* ... Raymond H. Gusteson 829
Meyer, Marshall W. *Change in Public Bureaucracies* ... John L. Foster 830
Mower, A. Glenn, Jr. *The United States, The United Nations, and Human Rights* ... James A. Stegenga 831
Pfiffner, James P. *The President, the Budget, and Congress: Impoundment and the 1974 Budget Act* ... Harvey C. Mansfield, Sr. 831
Schlozman, Kay L., and Sidney Verba. *Injury to Insult: Unemployment, Class and Political Response* ... Richard A. Brody 832

Urofsky, Melvin I., and David W. Levy, eds. *Letters of Louis D. Brandeis: Vol. 4, 1916–1921: Mr. Justice Brandeis;* Vol. 5, *1921–1941: Elder Statesman* .... Robert G. Seddig 833
Wilson, Graham K. *Unions in American National Politics* .... Abraham Holtzman 834

Comparative Politics

Allinson, Gary D. *Suburban Tokyo: A Comparative Study in Politics and Social Change* .... John C. Campbell 835
Barbagli, M., et al. *Fluidità Elettorale e Classi Sociali in Italia: 1968–1976* .... Raymond Seidelman 836
Barry, Donald D., et al., eds. *Soviet Law After Stalin:* Vol. 1, *The Citizen and the State in Contemporary Soviet Law;* Vol. 2, *Social Engineering Through Law* .... Peter Vanneman 837
Bill, James A., and Carl Leiden. *Politics in the Middle East* .... Don Peretz 837
Chu, Godwin C., and Francis L. K. Hsu, eds. *Moving a Mountain: Cultural Change in China* .... Alan P. L. Liu 838
Dearlove, John. *The Reorganization of British Local Government: Old Orthodoxies and a Political Perspective* .... Jerry A. Webman 847
Dienel, Peter C. *Die Planungszelle* .... Charles R. Foster 839
Drucker, H. M. *Doctrine and Ethos in the Labour Party* .... Bruce E. Cain 840
Hawker, Geoffrey, et al. *Politics and Policy in Australia* .... Lawrence Mayer 841
Irving, R. E. M. *The Christian Democratic Parties of Western Europe* .... Arthur B. Gunlicks 842
Katzenstein, Mary F. *Ethnicity and Equality: The Shiv Sena Party and Preferential Policies in Bombay* .... Daniel R. Graves 843
Khalifa, Ali Mohammed. *The United Arab Emirates: Unity in Fragmentation* .... Roy E. Thoman 844
Kirp, David L. *Doing Good by Doing Little: Race and Schooling in Britain* .... Elizabeth H. Crighton 844
Kornberg, Allan, et al. *Citizen Politicians-Canada: Party Officials in a Democratic Society* .... Ronald G. Landes 845
Kramnick, Isaac, ed. *Is Britain Dying? Perspectives on the Current Crisis* .... Joel D. Wolfe 846
Lagroye, Jacques, and Vincent Wright, eds. *Local Government in Britain and France: Problems and Prospects* .... Jerry A. Webman 847
Linz, Juan J., and Alfred Stepan, eds. *The Breakdown of Democratic Regimes* .... Philippe C. Schmitter 849
McCauley, Martin. *Marxism-Leninism in the German Democratic Republic: The Socialist Unity Party (SED)* .... Fred H. Eidlin 852
Mitra, Subrata Kumar. *Governmental Instability in Indian States: West Bengal, Bihar, Uttar Pradesh and Punjab* .... Richard Sisson 853
Rosenberg, David A., ed. *Marcos and Martial Law in the Philippines* .... Robert L. Youngblood 854
Roxborough, Ian, et al. *Chile: The State and Revolution* .... Arturo Valenzuela 855
Rugh, William A. *The Arab Press: News Media and Political Process in the Arab World* .... Ibrahim Abu-Lughod 857
Schultz, Ann. *Local Politics and Nation-States: Case Studies in Politics and Policy* .... Donald B. Rosenthal 858
Shah, Ghanshyam. *Protest Movements in Two Indian States: A Study of the Gujarat and Bihar Movements* .... Kul B. Rai 859
Sharkansky, Ira. *Wither the State? Politics and Public Enterprise in Three Countries* .... Gary Freeman 859
Shaw, Timothy M., and Kenneth A. Heard, eds. *The Politics of Africa: Dependence and Development* .... Rodger Yeager 860
Sigmund, Paul E. *The Overthrow of Allende and the Politics of Chile, 1964–1976* .... Arturo Valenzuela 855
Somjee, A. H. *The Democratic Process in a Developing Society* .... Marcus Franda 862
Stallings, Barbara. *Class Conflict and Economic Development in Chile, 1953–1973* .... Brian Loveman 862
Stepan, Alfred. *The State and Society: Peru in Comparative Perspective* .... Robert A. Packenham 864
Tökés, Rudolf L., ed. *Opposition in Eastern Europe* .... James P. McGregor 866
Valenta, Jiri. *Soviet Intervention in Czechoslovakia, 1968: Anatomy of a Decision* .... Edward Taborsky 867

Vardys, V. Stanley. *The Catholic Church, Dissent and Nationality in Soviet Lithuania* . . . . . Vasyl Markus 867
Walkland, S. A., ed. *The House of Commons in the Twentieth Century: Essays by Members of the Study of Parliament Group* . . . . . Harris N. Miller 868
Wright, A. W. *G. D. H. Cole and Socialist Democracy* . . . . . G. David Garson 870
Wright, Vincent, ed. *Conflict and Consensus in France* . . . . . Roy Pierce 871

International Politics

Adams, Walter, et al. *Tariffs, Quotas and Trade: The Politics of Protectionism* . . . . . Wayne Lewis 873
Bobrow, Davis B., et al. *Understanding Foreign Policy Decisions: The Chinese Case* . . . . . Thomas J. Price 873
Evans, Ernest. *Calling a Truce to Terror: The American Response to International Terrorism* . . . . . Edward Mickolus 874
Foster, Richard B., et al., eds. *Strategy and Security in Northeast Asia* . . . . . Hong N. Kim 876
Grove, D. John, ed. *Global Inequality: Political and Socioeconomic Perspectives* . . . . . Walker Connor 877
Kaufman, Edy, et al. *Israel-Latin American Relations* . . . . . Edward B. Glick 878
Laqueur, Walter. *A Continent Astray: Europe 1970–1978* . . . . . Martin O. Heisler 879
Lauren, Paul G., ed. *Diplomacy: New Approaches in History, Theory, and Policy* . . . . . Joan K. Wadlow 880
McNeill, William H., and Ruth S. Adams, eds. *Human Migration: Patterns and Policies* . . . . . Rosemarie Rogers 881
Mellor, John W., ed. *India: A Rising Middle Power* . . . . . Theodore P. Wright, Jr. 882
M'Gonigle, R. Michael, and Mark W. Zacher. *Pollution, Politics, and International Law: Tankers at Sea* . . . . . Louis R. Beres 883
Osgood, Robert E. *Limited War Revisited* . . . . . Abbott A. Brayton 884
Petras, James. *Critical Perspectives on Imperialism and Social Class in the Third World* . . . . . Eldon Kenworthy 885
Shichor, Yitzhak. *The Middle East in China's Foreign Policy 1949–1977* . . . . . Peter Van Ness 886
Smouts, Marie-Claude. *La France à l'ONU: Premiers Rôles et Second Rang* . . . . . Robert S. Wood 888
Sutter, Robert G. *China-Watch: Toward Sino-American Reconciliation* . . . . . William F. Dorrill 889
Villamil, José J., ed. *Transnational Capitalism and National Development: New Perspectives on Dependence* . . . . . Pedro A. Cabán 890
Vogel, Hans. *Der Kleinstaat in der Weltpolitik: Aspekte der schweizerischen Aussenbeziehungen im internationalen Vergleich* . . . . . Jürg Steiner 891
Warnecke, Steven J., ed. *International Trade and Industrial Policies: Government Intervention and an Open World Economy* . . . . . John Zysman 892
Waterbury, John. *Hydropolitics of the Nile Valley* . . . . . Carl Leiden 893

Reference Sources

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Feldbrugge, F. J. M., ed. *The Constitutions of the USSR and the Union Republics: Analysis, Texts, Reports* 894
Finer, S. E., ed. *Five Constitutions.* 894
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The 1978 CPS national election study, which includes many new questions about congressional candidates, is analyzed to discern what voters know about congressional candidates and why House incumbents are so successful at getting reelected by wide margins. Scholars have underestimated the level of public awareness of congressional candidates, primarily because of faulty measures. Voters are often able to recognize and evaluate individual candidates without being able to recall their names from memory. Incumbents are both better known and better liked than challengers, largely because they have the resources enabling them to communicate with their constituents frequently and directly. Yet the seriousness of the challenger is equally important for understanding the advantages of incumbency and why incumbency is less valuable in the Senate than in the House. Finally, public assessments of the president provide a national dynamic to congressional voting, but the effect is modest compared to the salience of the local choices.

During the first generation of research on voting behavior, scant attention was paid to congressional elections. In the 1970s scholars interested in congressional elections quickly exhausted the analytic possibilities of data from the principal source of nationwide sample surveys, the biennial National Election Studies of the University of Michigan Center for Political Studies. Their research provided some interesting findings but also pointed to a need for more and better survey items to explain the gaps and contradictions in the existing state of scholarly understanding.

The Michigan CPS study of the 1978 elections included a number of new questions about public perceptions of congressional candidates and incumbent members of Congress; for the first time scholars have data with which to find valid answers to some of the fundamental questions about how members of Congress are elected. In this article we will exploit the new data source to provide answers to these questions: What do voters know about congressional candidates? How do voters decide between candidates? Why are House incumbents so successful at getting reelected by wide margins? Why do Senate incumbents fare less well? How are elections won or lost?

The 1978 National Election Study

The unusually widespread scholarly interest in the 1978 congressional elections stems from the availability of a distinctly new data set rather than from any unusual features of the results. The outcome of the election can be briefly summarized. The Democratic party maintained the majority status in both houses that it has enjoyed without interruption for over two decades. The national net Republican gain in the House of Representatives of 2.8 percent of the votes and 3.4 percent of the seats was modest by midterm election standards, but not wholly unexpected, given several mitigating factors: the absence of presidential coattails in 1976 (Jimmy Carter ran behind 270 of the 292 Democrats elected to the House in that year); Carter's partial and temporary recovery in the opinion polls following his Middle East Camp David summit meeting; and the electoral advantage of incumbency. Twenty-four House incumbents seeking reelection were defeated—5 in the primaries and 19 in the general election—leaving 94 percent successful. Incumbency was a less obvious advantage in the Senate, where 10 of the 25 senators seeking reelection were defeated—three in the primaries and seven in the general election.[1]

The 1978 elections witnessed the continuation of three additional trends that have been present for several successive elections. First, turnout declined to a postwar low of 35.1

The data used in this paper were made available by the Inter-University Consortium for Political and Social Research and were originally collected by the Center for Political Studies of the University of Michigan. Neither the ICPSR nor the CPS has any responsibility for our analyses or interpretations.

[1] Two of the three senators defeated in the primaries were short-term appointees filling vacant seats, as was one of the seven who lost their seats in the November election.

percent. Second, David Mayhew's (1974a) vanishing marginals vanished even further. In 1978, 76 percent of all House races contested by an incumbent were won by a margin of 60 percent or more. Third, the growing insulation of House incumbents from defeat has not led to stability in membership. To the contrary, 18 percent of the House were replaced following the 1978 elections. When added to the shifts following the 1974 and 1976 elections, this means that half the members of the 96th Congress were not in office when Richard Nixon resigned from the presidency. This is equally true for the Senate, where 20 new members entered following the 1978 election and where 49 are in their first term. The answer to this seeming paradox is obvious: retirement has replaced defeat as the primary source of turnover and the number of retirements has been rising since 1972. Once again, 1978 was no exception to this pattern—49 representatives and 10 senators declined to seek reelection. Both figures are as high as any in modern times.

The 1978 National Election Study is the first nationwide survey in 20 years to include more than the most basic questions concerning congressional elections. A substantial portion of the questionnaire was devoted to congressional matters. At the same time, the primary sampling units were changed from counties to congressional districts, which produced a more even distribution of respondents across districts than was the case for the Miller-Stokes 1958 study (Stokes and Miller, 1962). The addition of a substantial body of contextual data permits analysis with the district as well as the individual as the unit of analysis.

A better sense of the opportunities as well as the limitations of working with this new data set can be gained by looking at Table 1, which presents the number of respondents, voters, districts and states in the 1978 sample. In the House the major constraint is the total number of respondents in districts with contested races who reported voting for one of the major party candidates—757 where the incumbent ran and 116 where there was an open-seat contest.[2] There are enough voters in incumbent-contested races for most analytic purposes, but it should be remembered that there is an average of fewer than ten voters per district. The small number of voters in elections without incumbents prevents all but the most straightforward presentation of frequencies.

If one has a special interest in races where the outcome is not a foregone conclusion, the sample's limitations are more pronounced. As we indicated above, 24 percent of all House incumbents seeking reelection won with margins of less than 60 percent, which is matched almost perfectly in the sample with 18 "marginal" races out of 77 incumbent-contested races. Although the sample was right on target in this respect, the number of voting respondents in these districts is small for most analytic purposes (178, of whom 170 are major-party voters). Moreover, there is a clear incumbent bias in the voting reports of these respondents. While challengers in all marginal districts averaged 46 percent of the actual vote, less than 35 percent of all the respondents in the sample of these districts reported a vote for the challenger. Any interpretation of the public's view of challengers in competitive districts must take this discrepancy into account.

Actually, an over-report of the vote for incumbents in the sample is not limited to marginal districts. In the universe of districts in which they faced opposition, incumbents won 66 percent of the major-party vote; yet in the CPS sample of these districts 79 percent of the vote was reportedly cast for incumbents.

Only one of the 19 districts in which incumbents were defeated fell within the sample; by chance, we would have expected four or five. We cannot expect the sample to represent such districts; nor would their inclusion have altered substantially the figures for the entire sample. Nonetheless, it is worth observing that a politically and analytically important type of district is under-represented in the sample.

The Senate sample poses much more serious problems. There is no purposive sample of Senate races; what we have is a by-product of designing a national sample of noninstitutionalized adults and of congressional districts. Roughly half the respondents were in states without a 1978 Senate contest. Moreover, the remaining respondents are distributed *very unevenly* across states with Senate elections, ranging from 19 in Iowa to 152 in Texas. There are two major limitations in the data on states where an incumbent senator sought reelection: (1) Fewer than 400 respondents reported casting a vote for a major party candidate. (2) Most of these respondents lived in Illinois, Michigan, or Texas, states in which senior Republican senators were engaged in expensive battles for survival. There are not enough cases for study of any one of these three races, and at the same time we feel uneasy about using respondents from these three states as the principal basis for generalizations about Senate elections. We

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[2] Nine respondents voted for third-party candidates in incumbent-contested elections, as did three respondents in districts with open-seat races.

Table 1. Respondents, Voters, Districts and States in the 1978 National Election Study, by Type of Election

	Districts or States	Total Respondents	Total Voters*	Democratic Voters	Republican Voters
House of Representatives					
Total	108	2304	1027	590	419
Contested races	88	1803	885	479	394
Democratic incumbent	47	917	432	342	88
Republican incumbent	30	651	334	74	253
Open seat	11	235	119	63	53
Uncontested races	20	501	142	111	25
Democratic incumbent	13	335	103	99	–
Republican incumbent	5	126	26	–	25
Open seat	2	40	13	12	–
Senate					
Total	39	2304	556	284	273
No race	13	1092	–	–	–
Contested races	24	1116	550	272	273
Democratic incumbent	8	256	122	82	39
Republican incumbent	8	558	274	116	157
Open seat	8	302	154	73	77
Uncontested races	2	96	16	12	0
Democratic incumbent	1	37	–	–	–
Republican incumbent	0	–	–	–	–
Open seat	1	59	16	12	0

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

*Includes all respondents who identified the candidate for whom they voted, not just major-party voters.

think it best to use the Senate figures for only the simplest comparisons with the House. This article primarily concerns voting in House elections.

The Importance of Party

Because members of Congress are less familiar than the president to most voters, it is easy to accept the proposition that party identification is a far more important determinant of congressional than of presidential voting. This notion can be evaluated by examining Table 2, which displays the proportion of voters in every House and presidential election from 1956 through 1978 who were pure Independents, people who identified with a party and voted for that party's candidate, and people who identified with one party and voted for the candidate of the other party.[3]

The first observation to be made is the marked increase in defection in presidential elections in 1968 and 1972, and then the equally sharp return in 1976 to the same modest levels of defection that were found during the 1950s. The trend toward weakening party lines that was so widely discussed in literature based on the 1968 and 1972 elections may well have reflected the candidacies of George Wallace and George McGovern rather than any secular movement in the strength of party loyalties (Nie et al., 1976).

Second, and more interesting for our present purposes, is the continuous increase in the proportion of congressional voters who defect. In 1956, just 9 percent of all those people casting a ballot in a House contest voted for the other party's candidate, compared with 15 percent of presidential voters. The proportion of House defectors crept up slowly and steadily

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[3] People who initially call themselves Independents and then (in response to the follow-up question) admit that they lean toward one or the other party think and vote like outright identifiers with that party rather than like people who insist that they are Independents (Keith et al., 1977). Therefore we have classified these Independent Democrats and Independent Republicans with the relevant party here and throughout this article.

Table 2. The Composition of the Vote in Presidential and House Elections, 1956–1978 (Percent)

	House Elections			Presidential Elections		
	Party-Line Voters[a]	Defectors[b]	Pure Independents	Party-Line Voters[a]	Defectors[b]	Pure Independents
1956	82	9	9	76	15	9
1958	84	11	5			
1960	80	12	8	79	13	8
1962	83	12	6			
1964	79	15	5	79	15	5
1966	76	16	8			
1968	74	19	7	69	23	9
1970	76	16	8			
1972	75	17	8	67	25	8
1974	74	18	8			
1976	72	19	9	74	15	11
1978	69	22	9			

Source: National Election Studies, Center for Political Studies, University of Michigan.

[a] Party identifiers who vote for the candidate of their party.

[b] Party identifiers who vote for the candidate of the other party.

in virtually every election since 1956. Until the most recent elections, however, defection was always lower than in presidential races, and so the contrast between presidential and House voting remained consistent with the proposition that ignorance about House candidates led to greater party-line voting than in presidential balloting, where public familiarity with the candidates produced more defection. But more recently the continued increase in House defection combined with a return to party-line presidential voting to reverse the previous relationship. In 1976, just 15 percent of presidential voters defected; by 1978, a substantially larger number of House voters—22 percent—identified with one party and voted for the other party's candidate. This reversal, whether temporary or permanent, is sufficiently damaging to the conventional wisdom to require further investigation.

Incumbency as an Electoral Force

Defection has not increased in a politically neutral manner. Cover and Mayhew (1977) found that the proportion of defections in favor of House incumbents rose substantially after 1970 and accounted for over three-quarters of all votes cast against party lines in House races. Even in 1974, when national tides ran strongly against the Republican party and 36 Republican representatives were defeated, 74 percent of all defections favored the incumbent. By 1978, challengers received only 11

Table 3. Vote for House and Senate, by Party Identification and Incumbency, 1978 (Percent)

	Party Identification of Voter		
	Incumbent's Party	Independent	Challenger's Party
Vote for House			
Incumbent	95	79	54
Challenger	5	21	46
n =	(407)	(72)	(277)
Vote for Senate			
Incumbent	89	64	31
Challenger	11	36	69
n =	(179)	(36)	(179)

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

percent of the defections; two decades earlier the comparable number was 43 percent.[4] These findings suggest that the secular decline in party-line voting observed in Table 2 reflects the greater ability of incumbents to attract votes from people who identify with the other party.

The relationship between party, incumbency and vote choice in the 1978 elections can be seen more directly in Table 3. Party and incumbency together appear to be an almost unbeatable combination in House elections. Only 5 percent of the voters (19 respondents) who belonged to the same party as their incumbent representative (hereafter called "incumbent partisans") voted for the challenger. When party and incumbency are in conflict, the latter appears as the more powerful electoral force. By contrast, incumbency has a somewhat weaker influence in Senate voting.

Before exploring the source of this incumbency advantage, we believe it is worth introducing here a theme to which we return later. The figures reported in the last paragraph are national averages, and they reflect the overwhelming advantage incumbents enjoy nationally. Yet House elections are held in individual congressional districts and the relative influence of party and incumbency may vary from one district to another. One study based on surveys in a number of congressional districts (most of which were in the "marginal" range at one point or another during the last six years) found that some challengers attracted as many as 20 percent of the votes of incumbent partisans, while some incumbents received only 14 percent of the support of challenger partisans (Mann, 1978). Of course, these examples are not representative of districts generally, but no discussion of voting in congressional elections is complete without a consideration of when and how challengers make the best showing against incumbents. Without this understanding, we are in no position to learn the conditions under which change might occur. But with only 19 incumbent partisans voting for challengers in the 1978 data set, there is no variance to investigate. Nonetheless, this need not prevent us from looking for processes of individual decision making that, when certain conditions change, might produce very different results.

The source of the increased incumbency advantage that was registered in the 1960s has been a puzzle to scholars. David Mayhew (1974b) originally suggested three possible explanations: (1) incumbents benefited from favorable redistricting that increased the number of majority partisans in their districts; (2) incumbents gained political support by becoming more adept at advertising, credit-taking, and position-taking; and (3) incumbents benefited fortuitously from the erosion of party loyalties–incumbency replaced party as a voting cue. Mayhew did not explicitly mention a fourth possibility, not unrelated to his third, that the visibility and attractiveness of challengers decreased during this time.

Cover (1977) and Ferejohn (1977) have shown that redistricting does not account for the increased advantage of incumbency, although doubtless there are particular cases where redrawing district lines made incumbents safer. Investigation of the second and third explanations has been constrained by the lack of adequate data on public perceptions of congressional candidates. Cover (1977), Ferejohn (1977) and Nelson (1978) all show that the increase in incumbents' electoral advantages was not accompanied by any increase in the proportion of the population who could recall their names, which suggested that incumbency provided a voting cue unmediated by public awareness of the incumbent. The same conclusion came more directly from Nelson's finding that many voters defected to incumbents whose names they could not recall. This implausible possibility can now be safely dismissed, as we will soon see. But unraveling the other two possibilities–that incumbents are better known and/or better liked now than before, or that the advantage in visibility and reputation long held by incumbents, although not increased, now translates more profitably into votes because party ties are weaker–has been frustrated by the lack of appropriate recognition and reputation measures during the decade when the advantage increased. While we can never adequately investigate this period of change, we can begin to describe more accurately what the public knows about incumbents and challengers today.

Candidate Familiarity

Incumbents are better known than their opponents. Scholars have generally said that incumbents enjoy more "name recognition," and point to repeated findings that incumbents are known to about twice as many people as challengers. What the surveys invariably measure however, is not name *recognition* but name

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[4] Because the 1978 CPS sample over-reported the national vote for incumbents, defections favoring challengers probably were somewhat higher in 1978.

recall. The Michigan CPS (1975, p. 124) question in 1974 was: "Do you happen to remember the names of the candidates for Congress—that is, for the House of Representatives in Washington—that ran in this district last November?" Of course, it is far more difficult to recall someone's name without help than it is to recognize a name when it is encountered. Many voters unable to recall the names of their representatives or challengers can nevertheless recognize the names when presented with them, which is the situation they face in the voting booth (Mann, 1978). It seems reasonable to expect, then, that the apparent existence of an incumbent electoral advantage without public knowledge of the incumbent reflects a faulty measure of knowledge, not an ability to impress voters without their conscious awareness.

The 1978 Michigan study for the first time included a true measure of name recognition, one that resembled (although not exactly) the situation voters face in the polling place. This was done with the "candidate thermometer," which elicited responses to a number of public figures, including all major party congressional candidates as well as several national political celebrities. For each person on the list of more than 20 names, the respondents could indicate they did not recognize the name, that they recognized but could not rate the person, or that they recognized and rated the person (see appendix). The thermometer thus measures both name recognition and affect toward the person. Table 4 shows the percentage of voters in contested races who recognized and rated the House and Senate candidates, those who claimed to recognize but could not rate the candidate, and those who confessed they could not recognize the candidate.

We see that House incumbents are almost universally recognized by voters in their districts, while the names of challengers are familiar to just under two-thirds of the voters. The lead of incumbents over challengers is more pronounced when we use a more stringent standard of recognition—both recognizing and rating the candidate. Open-seat candidates are substantially more visible than those who challenge incumbents, confirming the general view that open seats are more hotly contested than incumbent-held seats. The contrast with the Senate is instructive, with a caveat for the peculiar nature of the Senate sample. Senate challengers are almost as visible to the electorate as Senate incumbents, probably reflecting both more serious efforts by the opposition and the publicity that comes automatically to a statewide challenger.

Although most voters know the names of challengers and almost all are familiar with the incumbent, this discrepancy alone does not explain the incumbent advantage. Incumbents' very high victory rate is not fully explained by their greater name familiarity. Challengers have an uphill fight even when the voters know them. This is revealed in Table 5, which displays defection by incumbency, party identification, and name recognition. Voters who belong to the incumbent's party are almost sure to vote for the incumbent. Even the handful who do not recognize their representative's name have only a one-in-seven chance of defecting. By the same token, the challenger whose name is known to the incumbent's partisans wins only 7 percent of their voters—and loses fully 46 percent of his or her own partisans. The challenger is better off being recognized, but visibility alone is not enough. Thus we must move beyond simple name recognition in order to understand the advantages of incumbency and the opportunities for challengers.

Candidate Reputation

The 1978 National Election Study interview schedule gave respondents several opportunities to evaluate the congressional candidates in their district and state. Responses to these questions are all consistent with the proposition that as a class, incumbents are well regarded by their constituents. Table 6 shows the percentage of voters who rated the candidates on the feeling thermometer as positive, neutral, negative, or unknown. Almost three-fourths rated the incumbent positively, while less than a fifth made similar evaluations of the challenger.

One can better understand the advantage of incumbency in House elections by contrasting these figures with those for candidates in open House seats and for Senate incumbents and challengers. In the latter cases, the standing of the non-incumbents is relatively high and the gap between the two candidates is relatively narrow.

In addition to thinking highly of incumbents' personal qualities, most voters also feel that their representative is doing a good job. The two sorts of assessment are not always similar. (Sometimes the public combines generalized admiration for a political figure and a low assessment of performance. Jimmy Carter and Gerald Ford come immediately to mind.) Table 7 presents the job ratings of House members by party identification and the amount of defection at each category of performance assessment. These figures confirm the well-known point that individual members are

Table 4. Candidate Recognition Among Voters in Contested Races, House and Senate, 1978 (Percent)

	Recognize and Rate	Recognize, but not Rate	Don't Recognize
House			
Incumbents (n = 754)	93	4	3
Challengers (n = 754)	44	19	37
Open seat candidates (n = 116)	72	20	8
Senate			
Incumbents (n = 408)	96	3	1
Challengers (n = 408)	86	7	7
Open seat candidates (n = 158)	88	8	5

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

highly esteemed despite the public's critical view of Congress as an institution (Fenno, 1975). Nearly two-thirds of all voters think their representative is doing a "good" or "very good" job, and a mere 4 percent have a distinctly unfavorable view. As we would expect, these assessments are affected by party identification; the incumbent's partisans are more enthusiastic than members of the other party. The more important finding, however, is that half the challenger's partisans have a favorable view of the incumbent's performance, and only 8 percent say the incumbent is doing a "poor" or "very poor" job.

The relationship between job assessment and defection is about as strong as we could expect, particularly in light of the handful of incumbent partisans who defected. Incumbents lost no votes at all from members of their own party who say they are doing a "very good" job, and very few from those who rate them "good" or "fair." The numbers are much more striking among challenger partisans, whose support for the incumbent ranges from 89 to 11 percent, depending on their evaluation of his performance. Support for the challenger among pure Independents is similarly related to the incumbent's job rating.

A final measure of candidate affect comes from the open-ended questions asking respondents if there is anything they like and dislike about the Democratic and Republican House candidates. Predictably, voters' party affiliations shape their answers. Whether incumbents or challengers are the object of the query, the incidence of favorable comments is about 15 percentage points higher among their own partisans. This modest difference is overshadowed by the far stronger relationship of incumbency to favorable assessment, as Table 8 shows. Only a fifth of the voters in the challenger's party had anything good to say about the challenger, while three times as many of them expressed favorable opinions about the incumbent in the other party.

The consequences of these assessments for the vote can be seen in the bottom line of Table 8. This shows both the importance of candidate image in congressional voting decisions and incumbents' vastly greater success in creating

Table 5. Party Defection by Incumbency and Candidate Recognition, 1978 House Elections (Percent)*

	Voter Recognizes**			
	Incumbent		Challenger	
Party Identification	Yes	No	Yes	No
Incumbent's party	4	14	7	3
n =	(366)	(29)	(161)	(243)
Challenger's party	57	15	46	63
n =	(257)	(20)	(145)	(132)

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

*The entry in each cell is the percentage of voters with the indicated characteristics who defected.

**Defined as able to rate candidates on thermometer from 0–100.

Table 6. Voters' Feelings About Candidates, 1978 (Percent)

	Feeling Thermometer Evaluation			
	Positive	Neutral	Negative	Can't Rate; Don't Recognize
House				
Incumbents (n = 754)	73	10	9	7
Challengers (n = 754)	17	18	9	56
Open-seat candidates (n = 116)	50	13	9	28
Senate				
Incumbents (n = 408)	61	15	20	4
Challengers (n = 408)	48	17	20	15
Open-seat candidates (n = 158)	58	16	14	12

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

favorable impressions. Differences in defection rates in this table are more pronounced, especially for challengers, than those associated with simple name recognition (Table 5). Thus the advantage of incumbents reflects more than just their ability to get their names before the public. It seems likely, moreover, that defection rates would be less loaded toward incumbents if more challengers could make themselves favorably known to voters.

Two final notes on the open-ended comments about the candidates. First, the number of respondents who disliked something about the incumbent or the challenger was relatively small—18 percent for the former, 12 percent for the latter—but higher, particularly for the incumbent, than appeared on either the feeling thermometer or the job rating. Second, the content of the open-ended remarks was highly personalized, with very few references to party, ideology, or issues. Voters appear to judge candidates, and incumbents in particular, on the basis of their perceived character, experience, and ties to the local community. Issues and ideology are subordinated to these personal and particularistic concerns.

Candidate Preference

The evidence presented above makes clear that voting in congressional elections is strongly associated with evaluations of the candidates. Yet up to this point we have analyzed vote choice only as it relates to the assessment of a single candidate. A more likely decision process is for the voters to evaluate both candidates at the same time and to choose the more attractive one. It is possible that a number of voters who rate the challenger positively have an even higher opinion of the incumbent. The reverse is less likely in view of the challenger's lower visibility, but it is possible. In order to deal with this possibility, we must include comparative candidate preferences, not just individual candidate ratings.

Table 9 displays the vote for incumbent and

Table 7. Incumbent Job Ratings, and Defection by Voters in House Elections, 1978 (Percent)

	Rating of Incumbent's Performance					
	Very Good	Good	Fair	Poor	Very Poor	Don't Know
Percent of all voters rating incumbent	20	44	22	3	1	10
Percent defecting from:						
Incumbent's party	0	4	12	–	–	8
n =	(113)	(180)	(68)	(3)	(2)	(38)
Challenger's party	89	75	35	11	–	21
n =	(28)	(114)	(83)	(19)	(4)	(29)
Percent of Independents voting for challenger	0	11	27	–	–	30
n =	(7)	(36)	(15)	(3)	(1)	(10)

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

Table 8. Favorable Comments About House Candidates, Party Identification, Incumbency, and Defection, 1978 (Percent)

	Incumbent				Challenger			
	Same Party		Other Party		Same Party		Other Party	
Percent liking something about the candidate	71		56		20		6	
n =	(407)		(277)		(277)		(407)	
	Yes	No	Yes	No	Yes	No	Yes	No
Percent defecting	2	12	72	30	16	63	20	3

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

Note: "Yes" and "No" refer to whether the respondent likes anything about the candidate in the given category.

challenger by party identification and candidate preference, following a format developed by Richard Brody (1976) for the study of presidential elections. Candidate preference is determined by the relative ranking of the incumbent and challenger on the feeling thermometer. "Don't recognize" and "can't rate" responses are treated as neutral; in cases where one candidate is not recognized or rated, feelings toward the other candidates are decisive. A respondent who does not recognize the challenger but who rates the incumbent negatively is coded as preferring the challenger.

The figures in Table 9 confirm the appropriateness of the candidate preference model for congressional voting: 94 percent of all respondents who prefer the incumbent vote for the incumbent; 88 percent of all respondents who prefer the challenger vote for the challenger. In cases where neither candidate is preferred, party identification is the best predictor of the vote, although incumbents fare better among challenger partisans in this situation than we might expect. This is probably best explained by our coding conventions—incumbents who were recognized but rated as neutral on the thermometer were treated the same as challengers who were not recognized, although the former have an edge in recognition.

With the data in Table 9 we can divide the vote for incumbents and for challengers into four categories: votes consistent with both party identification and candidate preference, those consistent with candidate preference only, those consistent with party identification only, and those consistent with neither. The results of this exercise, displayed in Table 10, demonstrate that routine party support not backed up by express preference for the party's candidate provides a small part of incumbents'

Table 9. Vote in 1978 House Election by Party Identification and Candidate Preference

	Party Identification							
Candidate Preference* and Vote	Incumbent's Party		Independent		Challenger's Party		Total	
	N	%	N	%	N	%	N	%
Prefer incumbent								
Incumbent	258	99	31	91	103	84	392	94
Challenger	2	1	3	9	20	16	25	6
Prefer neither								
Incumbent	123	95	25	81	45	47	193	75
Challenger	7	5	6	19	50	53	63	25
Prefer challenger								
Incumbent	7	41	1	14	2	3	10	12
Challenger	10	59	6	86	58	97	74	88
Total								
Incumbent	388	95	57	79	150	54	595	79
Challenger	19	5	15	21	128	46	162	21

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

*Determined by the relative ranking of the incumbent and challenger on the feeling thermometer.

Table 10. Relationship of Party Identification and Candidate Preference to Each Candidate's Total Vote, House, 1978 (Percent)

Vote Consistent With:	Incumbent	Challenger
Party ID and candidate preference	44	36
Candidate preference only	23	10
Party ID only	22	43
Neither*	12	11
	100	100
n =	(595)	(162)

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

*Vote is consistent with neither party identification nor candidate preference.

total support (22 percent), and a substantially larger part for that of challengers (43 percent). This reflects the failure of most challengers to build a substantial personal following. Yet, overall, most party-line votes are accompanied by a preference for the party's candidate; when partisanship and candidate preference conflict, voters are likely to defect from their party.

This analysis of candidate preference is, of course, colored by the relatively low visibility and standing of challengers in our national sample. The 17 cases in the lower left-hand cell of Table 9—where incumbent partisans prefer the challenger—are not sufficient to produce confidence in our finding that candidate preference for challengers will override partisanship to the detriment of incumbents. As additional support for our conclusion, therefore, we refer readers to a similar investigation based on surveys in a sample of generally competitive districts (Mann, 1978). That study confirms the ability of more visible and attractive challengers to garner the support of substantial numbers of incumbent partisans.

Why Incumbents are Well Known and Well Liked

We have argued that voting in a congressional election primarily involves choosing between competing candidates; party plays an important role for some voters in determining whether a candidate is attractive and in providing guidance for voters who lack information about the candidate, but it is secondary to the images of the candidates themselves. Incumbents have an enormous advantage over challengers, not because the voters' decision rules are rigged in their favor, but rather because they are more visible and more attractive. Why are members of Congress in such a desirable position?

The data in Table 11 move us a long way toward an explanation. Each figure in this table represents the percentage of all voters who had the designated form of contact with incumbents, challengers, or open seat candidates, in the House and Senate. When contrasted with the image of relative anonymity that appeared in the 1958 study of congressional elections (Stokes and Miller, 1962) the figures in this table, especially for incumbents, are staggering. Only 10 percent of all voters had no form of contact with the House incumbent. Almost a fourth met their representative personally, while nearly three-fourths had seen press coverage about him or her via the print media and/or television. By contrast, only 44 percent of these voters had any type of contact with the challenger. The gap between incumbent and challenger is greatest in mail contacts, which probably reflects the enormous advantage members of the House enjoy in blanketing their districts with mass and specialized mailings. The prohibition on franked (free) mass mailing by representatives for 60 days before an election evidently does not erase the effects of this perquisite of office.

The more one examines Table 11, however, the less impressive one finds the argument that the inherent advantages of office adequately explain incumbents' greater visibility. Candidates for open House seats reached a far larger share of voters than challengers did, and in many types of contact were nearly as successful as incumbents. One way or another, open-seat candidates reached 73 percent of the voters, compared to 90 percent for incumbents and 44 percent for challengers. The putative advantages of office are even less evident in Senate races, where voters have nearly as much contact with challengers and open-seat candidates as with incumbents. (Amount of contact is measured by numbers of voters reached, not the number of separate interactions.)

We believe that House incumbents benefit more from the scarcity of serious challenges than from the perquisites of office. Evidence on this point can be found in the data on House and Senate campaign expenditures displayed in Table 12. We assume that budgets are a reliable index of total campaign effort, particularly for non-incumbents who must pay for many services that are free to sitting members. The generous spending of candidates for open House and Senate seats helps explain our earlier findings about their visibility to the electorate. In races contested by incumbents, however, the campaign effort and consequently, we believe, the visibility of challengers varies enormously.

Table 11. Voter Contact with House and Senate Candidates, 1978 (Percent)

	House			Senate		
Form of Contact	**Incumbent**	**Challenger**	**Open Seat**	**Incumbent**	**Challenger**	**Open Seat**
Any type	90	44	73	94	82	88
Met personally	23	4	14	9	5	9
Saw at meeting	20	3	13	10	5	13
Talked to staff	12	2	13	6	4	9
Received mail	71	16	43	53	32	47
Read about in newspaper, magazine	71	32	57	73	63	78
Heard on radio	34	15	28	45	37	49
Saw on TV	50	24	48	80	70	78
Family or friend had contact	39	11	26	–	–	–
n =	(756)	(756)	(121)	(409)	(409)	(158)

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

Just over 70 percent of these elections were won by an incumbent with more than 60 percent of the vote. These campaigns were low-budget productions on both sides. The spending was even more lopsided than the voting, with challengers being outspent three-to-one by the victorious incumbents. Budgets were far larger in the 90 House races decided by smaller margins, particularly for the challengers, who spent on average five to six times as much as their counterparts in non-competitive races.

The pattern is similar in Senate contests—challengers in the closer races spend vastly more than challengers who are defeated decisively—but the frequencies are quite different. Most Senate challengers are big spenders and run competitive races, while the vast majority of House challengers have small budgets and even smaller chances of winning.

The absence of serious challenges to most House incumbents surely owes something to their public standing and their resourcefulness in exploiting the advantages inherent in their office. We suspect that many decisions not to

Table 12. Mean Campaign Expenditures in House and Senate Races, 1978

All Races	**House**	**Senate**
Open-seat candidates	$201,049	$ 820,787
n =	(111)	(25)
Incumbents	111,557[a]	1,341,942[b]
n =	(377)	(22)
Challengers	72,373	697,766
n =	(309)	(21)

Incumbent-Contested Races by Election Outcome	**House**		**Senate**	
	Incumbent	**Challenger**	**Incumbent**	**Challenger**
Incumbent won with 60% or more of vote	$ 93,218	$ 32,564	$ 453,772	$ 47,346
n =	(287)	(219)	(7)	(6)
Incumbent won with less than 60% of vote	161,856	156,445	2,496,484[c]	992,831
n =	(71)	(71)	(8)	(8)
Incumbent lost to the challenger	200,607	217,083	908,348	918,054
n =	(19)	(19)	(7)	(7)

Source: Computed by authors from Federal Election Commission data presented in *Congressional Quarterly Weekly Report,* September 29, 1979.

[a] Includes 68 uncontested races.

[b] Includes 1 uncontested race.

[c] Without the $7.5 million spent in Jesse Helms' narrowly successful reelection campaign in North Carolina, this figure would be $1.8 million.

wage a major challenge involve other factors as well, including the vitality of the local opposition party, the high cost of a serious campaign, the difficulty of raising enough money, and some degree of satisfaction with the incumbent among local interests. Moreover, some House districts have such a lopsided division of Republicans and Democrats that members of the minority party have little chance of winning under any circumstances. This is seldom true of Senate races because statewide constituencies generally are more evenly balanced between the two parties.

Table 11 also confirms other bits of conventional wisdom about House-Senate differences. Senators and those aspiring to the office are much more likely than their House counterparts to communicate with the electorate via broadcast media, especially television. Doubtless this reflects the better fit between statewide constituencies and television audiences. In contrast, many congressional districts are such a small portion of a metropolitan television viewing area that their representatives' activities are not very newsworthy. And when the media audience is so large, advertising becomes prohibitively expensive for a candidate who wants to reach only a fraction of it. Also, senators are bigger celebrities than representatives, a result of their smaller number and differences between the impersonal House and the Senate's star system.

The electoral payoff of contact with the voters can be seen from Table 13. Among voters who had contact only with incumbents, challengers lose 72 percent of their partisans and attract but 2 percent of the incumbent's partisans. Defection among challenger partisans is 26 percent lower among voters who have had contact with both candidates and 40 percent lower among the handful of respondents who have had contact with neither candidate. These latter results point to the importance of efforts by both incumbent and challenger.

In the face of these striking findings on the extent of candidate contact with voters, the increased advantage of incumbency clearly is not a result only of the errand boy or ombudsman activities of House members (Fiorina, 1977). Responses to a battery of questions on citizen-initiated contact with representatives let us assess more directly the electoral consequences of these activities. One-fifth of the voting respondents said that they had contacted their representative or someone in the representative's office—to express an opinion, to seek information, or to seek help with a problem, in roughly equal proportions. Virtually everyone received a response, and the vast majority were very satisfied with that response. Yet defection rates between those who contacted their representative's office and those who did not are almost identical. (Actually, the absence of a difference for all voters masks a partisan difference: Democratic incumbents do slightly better among those voters who *have not* contacted their office, while Republican incumbents do slightly better with those who *have* contacted their office.) If we look at just those respondents who wrote to seek help with a problem, we find that a large majority are incumbent partisans; and of the 19 challenger partisans who fell into this category, 11 voted for the incumbent, producing a defection rate *identical to that of all challenger partisans in* the sample. In other words, responding to constituent requests paid no special vote dividend for House members in 1978.

While the direct payoff for constituent service may have been limited, a reputation for service appears to have been more valuable. Many voters who have not themselves made requests of their congressmen know others who have, and this latter phenomenon is more

Table 13. Defection in 1978 House Elections, by Contact with Incumbent and Challenger (Percent)

	Contact with:			
Defection from:	Both Candidates	Incumbent Only	Challenger Only	Neither Candidate
Incumbent's party	8	2	–	8
n =	(155)	(213)	(0)	(38)
Challenger's party	46	72	–	32
n =	(145)	(103)	(1)	(28)
Support for challenger among Independents	29	7	–	–
n =	(34)	(30)	(1)	(6)

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

strongly related to the vote. Moreover, a third of the voters believe their representative would be "very helpful" if asked to help with a problem, and this reputation for service garners the vote of almost 80 percent of the challenger partisans. It is very likely that congressmen build a reputation for service in a variety of ways, exploiting vehicles for mass publicity without limiting themselves to direct servicing of constituent requests. In any case, the reach of other forms of contact with voters extends considerably beyond the constituent service domain.

One final note on additional ingredients in the positive image of incumbents. Richard Fenno (1978) has demonstrated that House members adopt a diversity of "home styles" in presenting themselves in their districts. Some members are highly issue-oriented in their discussions with constituents while others stress their personal identification with the district and make few references to substantive issues. In either case, however, Fenno believes that the result of such encounters with House members is an impression of their personal qualities rather than specific information on their issue positions or voting record. This proposition is confirmed by responses to items in the 1978 study that deal with the member's voting record or positions on issues. Only 15 percent of all voters remembered how their representative voted on any bill in the past couple of years. Fewer than half could say whether they generally agreed or disagreed with the way that representative voted in Washington. In either case, there was very little disagreement with the representative's actions, and the consequent loss in votes was trivial. These data strike a heavy blow at the argument that House members are likely to accumulate enemies because they must take visible stands on many controversial issues. Indeed, only a fraction of all comments about incumbents concern their positions on issues, and the vast majority of these are favorable.

There is somewhat wider awareness of the general ideological posture of representatives—80 percent of voters could place themselves on a liberal-conservative scale and 63 percent could place their congressman. However, only 26 percent were able to classify the challenger. Most ideological descriptions are accurate; Democrats are seen as predominately liberal, Republicans as conservative. We can gauge the importance of ideological proximity voting by looking at those voters who could place themselves and the two candidates on the scale—roughly a fourth of all voters. Table 14 displays the level of defection in House elections by the ideological distance of voters from the incumbent and challenger. The entries confirm the presence of proximity voting, but the sparse number of cases in each cell, particularly those where party and proximity conflict, demonstrate how minimal its effect is.

It would be a mistake, however, to conclude that issues and roll-call voting have no importance for congressional elections. The low salience of issues may reflect in part the efforts of the incumbent to avoid being dramatically out of step with district sentiment. In order to preserve a favorable public image, incumbents may act to forestall vociferous criticism on policy grounds. Incumbents must state positions on issues to satisfy local groups that are important for endorsements, campaign contributions, and volunteers. House members realize that the best way of ensuring their continued reelection is to discourage serious opposition. This requires making peace with those in the district who might otherwise underwrite a vigorous challenge. Finally, most politicians operate at the margin, not at the base. A loss of two or three percent of the vote as a result of a vote or a position taken on an issue will give them pause. Thus the small numbers of cases in Table 14 are not conclusive evidence against the proposition that taking positions on issues could affect the outcome of some House elections, and that the prospect of such an influence may well constrain members of Congress.

Midterm Elections as Referendums on Presidential Performance

Several scholars have presented evidence that voting in congressional elections is affected by public assessment of the president's performance. Members of the president's party who like the job the president is doing are less likely to vote for a House candidate of the other party. By the same token, people in the other party who like the way the president is behaving in office are more likely than their fellow-partisans to vote for a congressional candidate in the president's party (Arseneau and Wolfinger, 1973; Kernell, 1977). Using aggregate data, Tufte argues that the size of the losses sustained by the president's party in midterm elections depends on his popularity and how the economy is faring (1975, 1978). Actually, this latter theory predicts only changes in the partisan division of the national vote for the House of Representatives, either from the preceding presidential year or from some standardized measure of the normal midterm vote. Consequently, at the individual level,

Table 14. Party Defection, by Ideological Distance of Voters from House Candidates (Percent)*

	Voter's Ideological Position		
Party Identification	Closer to Incumbent	Equidistant from Candidates	Closer to Challenger
Incumbent's party	2	6	50
n =	(54)	(18)	(10)
Challenger's party	58	57	15
n =	(12)	(21)	(40)

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

*Includes only voters who classified their own ideological position and that of the two candidates.

we should expect to see its effects only at the margin. And in a year like 1978, when the national swing was less than 3 percent, we should find little evidence of voting in reaction to the president's performance. Moreover, since incumbency seems to insulate House members of the president's party somewhat from adverse judgments of the president's performance (Nelson, 1978), we might expect that the rising electoral effects of incumbency would reduce the referendum effect.

Table 15 shows how voting for the House varied according to respondents' judgments of President Carter's performance. Twenty-six percent of Democrats who disapproved of his performance defected, compared to 22 percent of those Democrats who liked his White House record. Republicans who liked the job Carter was doing voted for Democratic House contenders at a rate of 29 percent, compared to 21 percent for Republicans who disapproved. While these differences are miniscule compared to those based on the relative attractiveness of the congressional candidates discussed earlier, they are sufficient to account for the national shift that was observed in 1978. The bottom half of Table 15 demonstrates that differences in defection rates are somewhat larger when more refined measures of presidential performance are used.

This finding in no way contradicts a major conclusion of this article—that in deciding how to cast their ballots, most voters are influenced primarily by the choice of local candidates. But if we want to explain changes in the national division of the vote from one election to another, especially when those shifts are substantial, we must turn our attention to national forces. How the national swing then translates into swing at the district level and swing in the

Table 15. Two Measures of Carter's Job Performance and Voting in House Elections (Percent)

	Democrats		Independents		Republicans	
House Vote	Approve	Disapprove	Approve	Disapprove	Approve	Disapprove
Democrat	78	74	51	47	29	21
Republican	22	26	49	53	71	79
	100	100	100	100	100	100
n =	336	98	45	19	106	185

	Democrats*				Republicans*			
House Vote	Very Good	Good	Fair	Poor	Good	Fair	Poor	Very Poor
Democrat	87	78	73	72	24	27	20	14
Republican	13	22	27	28	76	73	80	86
	100	100	100	100	100	100	100	100
n =	45	168	214	24	41	169	75	28

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

*There were too few Democrats giving "very poor" and Republicans giving "very good" ratings to be included in the table.

number of seats, and how important these national forces seem to candidates contesting in local districts, are other questions entirely.

Conclusion

Recent congressional elections can no more be accurately viewed as reflecting standing party decisions than can presidential elections. Most voters have sufficient information about at least one and often both of the candidates to base their decision on something other than party loyalty. Scholars have underestimated the level of public awareness of congressional candidates primarily because of faulty measures. Voters are often able to recognize and evaluate individual candidates without being able to recall their names from memory. At the same time, the content of these evaluations is both thin and highly personalized, with little apparent ideological or issue content.

Incumbents are both better known and better liked than challengers, which accounts for their obvious electoral advantage. No one activity of incumbents—such as the much-heralded ombudsman role on behalf of their constituents—is solely responsible for their popularity. A variety of activities that bring them to the attention of the voters in their districts pay handsome electoral dividends. Yet it is inaccurate to portray congressional voting as simply favorable or unfavorable (but mostly favorable) decisions on the incumbent. Equally important are the public visibility and reputation of the challenger. Most voters make their decision between candidates by judging which one they like better, not by pulling the party lever or turning automatically to the incumbent. In most districts the challenge to incumbents is so inconsequential that the largely favorable images of incumbents prevail. In a much smaller number of districts, challengers stage a strong enough campaign to make the task of choosing between candidates a meaningful exercise. The advantage of incumbency in House elections—in contrast to Senate elections—is enhanced by the absence of a serious challenge and the greater likelihood of a lopsided partisan balance in the smaller and more homogeneous House constituency. The explosion of congressional perquisites has enabled House members to communicate with their constituents far more frequently and directly; and the absence of meaningful party competition at the district level coupled with traditional patterns of local press reporting (Robinson, 1980) works against alternative sources of (negative) information about the incumbent.

Finally, public assessments of the president provide a national dynamic to congressional voting, but the effect is modest compared to the salience of the local choices.

These findings raise as many new questions as they answer old ones. One of us has begun to explore how the conditions of election and reelection outlined here influence the way members of Congress behave in Washington (Mann, 1978, 1980). We won't repeat those arguments. Instead, we conclude with a comment on the research agenda of those who study congressional elections.

If most elections to the House are won for lack of a contest, and the evidence for this is impressive, then we need to know much more about the conditions under which a serious challenge is waged. Incumbent popularity among the voters is certainly an important deterrent to potential opponents and to those who might underwrite their campaign. When an incumbent looks popular, the most attractive candidates are unlikely to waste their time and credibility in a challenge against apparently heavy odds. The same consideration probably keeps contributors' checkbooks closed. Yet local leaders are likely to take other factors into consideration as well, such as the incumbent's record on legislative matters of great interest to them. Fenno reports that incumbents consciously strive to forestall serious opposition. We need to know more about what they do to accomplish that objective, and how these efforts are reflected, if at all, among the mass public. These and other questions relating to the recruitment of candidates and the mobilization of resources should lead us to supplement national samples with surveys of individual districts as well as studies of leaders and of contribution patterns at the district level. If most incumbents win reelection before the campaign begins, we should start to find out what happens between elections.

Appendix

This is the candidate thermometer (or "feeling thermometer") question (A16) used in the 1978 Michigan study:

> I'd like to get your feelings toward some of our political leaders and other people who are in the news these days.
>
> I will use something we call the feeling thermometer, and here is how it works:
>
> I'll read the name of a person and I'd like you to rate that person using the feeling thermometer. Ratings between 50 degrees and 100 degrees mean that you feel favorable and warm toward the person. Ratings between 0

degrees and 50 degrees mean that you don't feel favorable toward the person and that you don't care too much for that person.

If we come to a person whose name *you don't recognize,* you don't need to rate that person. Just tell me and we'll move on to the next one.

If you do recognize the name, but you *don't feel particularly warm or cold* toward the person, you would rate the person at the 50 degree mark.

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A Comparison of Voting for U.S. Senator and Representative in 1978

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This article compares voting for U.S. senator and representative in 1978. Analysis of data from the Center for Political Studies 1978 Election Study reveals that incumbents were better known and more positively evaluated than challengers, but House incumbents enjoyed a much greater advantage than Senate incumbents. The invisibility of most House challengers was a serious obstacle to accountability in House elections. Senate challengers were much more visible to the electorate. In addition, ideology and party identification had a greater impact on evaluations of Senate candidates than on evaluations of House candidates. Evaluations of House incumbents appear to have been based largely on frequent positive contacts between voters and their representative. As a result, ideological voting was more prevalent in Senate elections than in House elections.

Recent elections have produced a wide disparity between the outcomes of House and Senate races involving incumbents. While House incumbents have continued to enjoy overwhelming success, incumbent Senators have been highly vulnerable. In both 1976 and 1978, less than 5 percent of incumbents seeking reelection to the House were defeated while close to a third of the incumbent senators seeking reelection lost their races. Despite having only a third of its seats at stake, the Senate actually had proportionately greater turnover in its membership than the House in the past two elections.

Several factors may help to explain the difference between House and Senate elections. A House district is more likely to be dominated by one party than a state. In addition, Senate challengers generally appear to wage more visible and effective campaigns than House challengers. Electoral accountability requires effective competition. Even if the voters are less than completely satisfied with an incumbent's performance, they may be reluctant to vote for a challenger who is an unknown quantity. Finally, voters may use different criteria to evaluate the performance of senators and representatives. Senators appear to be associated more often with controversial national issues than representatives, whose positions on such issues do not generally receive as much publicity. Their greater prominence in the media may result in senators having less control than representatives over the information which voters receive about their performance. Thus voters may receive more objective, or even critical information about individual senators than about individual representatives. Most of the information which voters receive about their representatives in the House probably comes from representatives themselves.

Data

The data used in this study come from the Center for Political Studies 1978 National Election Study. Post-election interviews were conducted with 2304 eligible voters in 108 congressional districts throughout the United States. For the first time since 1958, the survey instrument was designed explicitly for the purpose of analyzing voting in congressional elections. Many questions concerning perceptions and evaluations of individual House and Senate candidates were included in addition to more traditional items concerning the parties, issues, and national political figures. Therefore the 1978 election study is particularly well suited to our purpose.[1]

The data used in this study were made available by the Inter-University Consortium for Political and Social Research. The author accepts all responsibility for the analyses and interpretations included in this article. I would like to thank John McGlennon and Ronald Rapoport for their comments on an earlier draft of this paper. Support for the research on which this article is based was provided by a Summer Research Grant from the College of William and Mary.

[1] The wording of the questions used in this analysis can be found in the codebook for the 1978 election study available from the Inter-University Consortium for Political and Social Research (ICPSR, 1979). Since both the sample and the survey instrument were designed primarily to study House races, the data are not ideal for the purpose of comparing Senate and House voting. Some questions asked about House candidates were not repeated for the Senate candi-

Findings

As might be expected, incumbents were generally better known and more positively evaluated than challengers. There was, however, a substantial difference between House and Senate races. Table 1 shows that incumbency had a much greater impact on candidate evaluations and voting decisions in House races than in Senate races. In House races, incumbents were evaluated more favorably than challengers by all groups of voters, including those who identified with the challenger's party, and almost half of the supporters of the challenger's party voted for the incumbent. Senate challengers generally fared much better than their House counterparts, especially among their own party's voters and among independents. Altogether, 78 percent of the votes in contested House races favored the incumbent while only 60 percent of the votes in contested Senate races favored the incumbent.

Why did House incumbents do so much better than Senate incumbents? There appear to be two answers to this question. First, House incumbents are evaluated more positively than Senate incumbents. Among all respondents, 56 percent gave their House incumbent a positive rating on the feeling thermometer while only 8 percent gave the incumbent a negative rating. Among respondents in states with Senate races, 53 percent gave the Senate incumbent a positive rating while 16 percent gave the incumbent a negative rating. Even more striking, however, is the difference between House and Senate challengers. Only 14 percent of respondents gave a positive rating to the House challenger while 7 percent gave the challenger a negative rating. The vast majority of respondents (79 percent) had no opinion about the House challenger. In contrast, 40 percent of respondents gave the Senate challenger a positive rating while 16 percent gave the challenger a negative rating and 44 percent had no opinion. Thus, House incumbents are doubly advantaged. Not only do they receive overwhelmingly positive evaluations from their constituents, but also their opponents are almost invisible. In contrast, Senate incumbents face a larger group of dissatisfied voters and much more vigorous opposition.

Table 1. Incumbency, Candidate Evaluation, and Voting for House and Senate in 1978

	Democratic Incumbent	Open Seat	Republican Incumbent	Tau	(Sig.)
House					
Candidate evaluation[a]					
Democrats	+56	+29	−37	.45	(.001)
Independents	+41	− 6	−41	.44	(.001)
Republicans	+14	−34	−65	.43	(.001)
Vote[b]					
Democrats	95	81	53	.35	(.001)
Independents	79	38	18	.60	(.001)
Republicans	45	16	3	.41	(.001)
Senate					
Candidate evaluation					
Democrats	+37	+38	+ 9	.15	(.001)
Independents	+ 4	+ 4	−26	.16	(.001)
Republicans	−18	−23	−48	.16	(.01)
Vote					
Democrats	85	81	74	.10	(.05)
Independents	64	47	30	.27	(.001)
Republicans	35	22	10	.20	(.01)

Source: 1978 Election Study, Center for Political Studies, University of Michigan.

[a] Percentage more favorable toward Democratic candidate minus percentage more favorable toward Republican candidate on feeling thermometer.

[b] Percentage voting Democratic.

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dates. In addition, the number of respondents in states with Senate races is less than optimal and these respondents are concentrated in a relatively small number of heavily populated states. Nevertheless, these limitations do not seriously jeopardize the validity of the findings reported in this article.

Mayhew (1974) has argued that the popularity of House incumbents is a function of their ability to control the information which constituents receive about their performance through such activities as advertising, credit-claiming, and position-taking. If this is the case, we would expect voters' evaluations of House incumbents to be based largely on contact with their representative, rather than ideology or party affiliation. The more contact voters have with their representative, the more positive their evaluation should be. If, as hypothesized, senators have less control over information voters receive about their performance than representatives, contact may not have as positive an impact on evaluations of Senate incumbents.

The 1978 election study included questions about various types of contact with candidates for the House and Senate. Table 2 shows high levels of contact between respondents and House incumbents. High levels of contact were also reported with Senate incumbents, although these contacts followed a somewhat different pattern, with more media contact and less personal contact. Once again, there was a large discrepancy between House and Senate challengers. Largely because of the influence of the mass media, respondents reported fairly high levels of exposure to Senate challengers. In contrast, the large majority of respondents reported having had no contact with the House challenger in their district.

In order to assess the relative impact of party identification, ideology, and contact on evaluations of House and Senate candidates, a path analysis was conducted, using relative ratings of incumbents and challengers on the feeling thermometer as the dependent variable. The independent variables were overall incumbent contact, overall challenger contact, party identification, and a measure of ideological proximity to the incumbent based on the respondent's self-classification on a seven-point liberal-conservative scale and the incumbent's ADA rating (see Appendix). The results shown in Figure 1 indicate that evaluations of House and Senate candidates were based on somewhat different criteria. Contact with the incumbent was by far the most important influence on evaluations of House candidates. The more respondents had seen or heard about the incumbent, the more highly they tended to evaluate the incumbent relative to the challenger on the feeling thermometer. Ideology had no discernible impact on evaluations of House candidates. In contrast, contact with the incumbent had a somewhat weaker impact on evaluations of Senate candidates, while party identification and ideology were more important.

Different types of contact may have different effects on voters' evaluations of candidates. Table 3 presents the results of a multiple regression analysis of the effects of contact on evaluations of incumbents and challengers. Since the dependent variable is the rating of the incumbent or challenger on the feeling thermometer, each unstandardized regression coefficient (B) can be interpreted as the average change in degrees on the feeling thermometer

Table 2. Contact with Incumbents and Challengers (Percent)

Type of Contact	House Incumbent	House Challenger	Senate Incumbent	Senate Challenger
Personal meeting	14	3	7	4
Group meeting	12	2	8	3
Staff	8	2	5	3
Mailing	52	13	41	22
Radio	27	11	40	30
Television	43	20	72	61
Newspaper	52	24	60	49
Indirect[c]	28	8	b	b
Self-initiated	15	a	b	a
Other-initiated[d]	19	a	b	a
Total reporting any contact	77	37	84	70

Source: 1978 Election Study, Center for Political Studies, University of Michigan.

Note: Percentages based on all respondents.

[a] Not relevant for challenger.

[b] Not asked about Senate candidates.

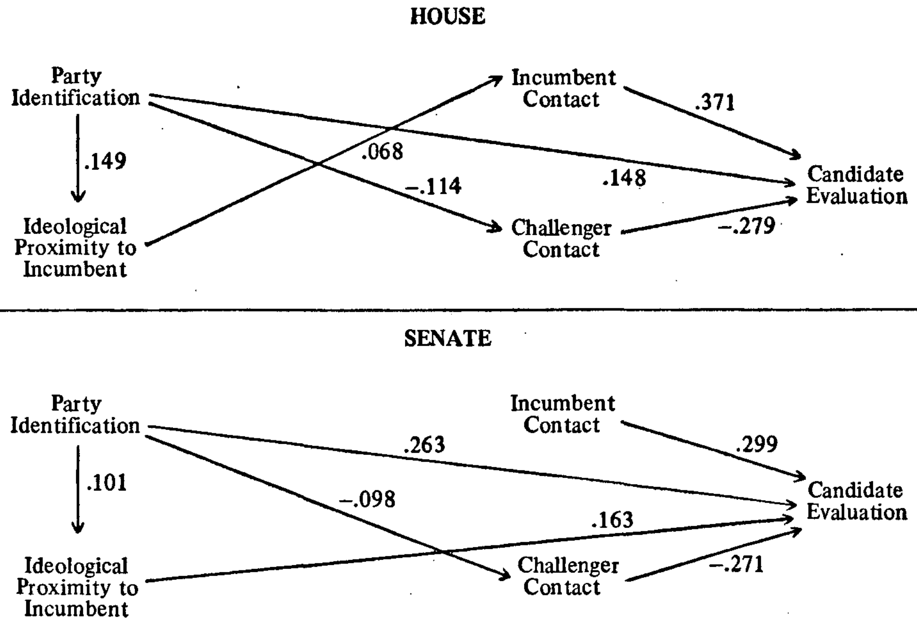
[c] Does respondent know of anyone else who has had contact with candidate?

[d] Does respondent know of anyone else who has initiated contact with candidate?

produced by each type of contact, controlling for all other types of contact. Multiplying this figure by the frequency of each type of contact indicates the net effect of each type of contact on the average evaluation of each type of candidate. These effects can then be summed to estimate the overall impact of contact for each type of candidate.

The results presented in Table 3 indicate that House incumbents derived considerably greater benefit from contact (a net gain of almost 10 degrees on the feeling thermometer) than any other type of candidate. Mailings (presumably newsletters and questionnaires as well as campaign literature) had the largest positive effect, followed by television and newspaper coverage. It is striking that every type of contact had a positive effect on evaluations of House incumbents, even after controlling for all other types of contact. In contrast, the impact of contact on evaluations of House challengers was negligible (a gain of only one degree on the feeling thermometer). The effects of contact on evaluations of Senate incumbents and challengers were almost identical, with television coverage having by far the largest positive impact on both. Presumably much of this television coverage consisted of paid advertising which has become a major factor in most Senate campaigns. If we remove the effects of contact, the advantage of incumbency in House elections was almost identical to the advantage of incumbency in Senate elections. More than two-thirds of the difference between ratings of House incumbents and challengers can be attributed to the effects of contact.

The fact that House incumbents enjoyed a much greater advantage than Senate incumbents over their challengers in 1978 was due in large part to the much lower visibility of House challengers. A much larger proportion of respondents reported having had some contact with, and having an opinion about, Senate challengers than House challengers. Unless they are aware of the challenger, voters who are dissatisfied with the incumbent may be reluctant to replace a known quantity with an unknown quantity. Table 4 lends support to this hypothesis. The relationship between evalu-


HOUSE
Party Identification
.149
Ideological Proximity to Incumbent
Incumbent Contact
Challenger Contact
.068
−.114
.148
.371
−.279
Candidate Evaluation
SENATE
Party Identification
.101
Ideological Proximity to Incumbent
Incumbent Contact
Challenger Contact
.263
−.098
.163
.299
−.271
Candidate Evaluation

Source: 1978 Election Study, Center for Political Studies, University of Michigan.

Note: All non-significant paths omitted. Weights shown are standardized regression coefficients. Analysis assumes no causal connection between incumbent and challenger contact.

Figure 1. Path Analysis of Candidate Evaluation in House and Senate Contests Involving Incumbents

ation of the incumbent and candidate choice in House and Senate elections was much stronger among voters who were exposed to the challenger than among those who were not. Among voters with a favorable opinion of the incumbent, contact with the challenger made little or no difference. However, among voters who had an unfavorable opinion or were undecided about the incumbent, contact with the challenger greatly increased the likelihood of voting against the incumbent.

The findings presented in Table 4 underline the crucial role that the challenger plays in the process of electoral accountability. Among voters in Senate races involving an incumbent, 83 percent reported having had some contact with the challenger. In contrast, only 44 percent of the voters in House races involving an incum-

Table 3. The Impact of Contact on Evaluations of House and Senate Candidates

Type of Contact	B	(S.E.)	Frequency	Net Effect
House Incumbents				
Personal meeting	+5.3	(1.4)	.14	+0.74
Group meeting	+4.2	(1.5)	.12	+0.50
Staff	+2.9	(1.7)	.08	+0.23
Mailing	+5.2	(0.9)	.52	+2.70
Radio	+0.9	(1.0)	.27	+0.24
Television	+3.6	(0.9)	.43	+1.55
Newspaper	+3.2	(1.0)	.52	+1.66
Indirect	+3.5	(1.1)	.28	+0.98
Self-initiated	+3.5	(1.3)	.15	+0.52
Other-initiated	+3.1	(1.2)	.19	+0.59
Intercept (54.00) + Total Effect (+9.71) = Average Rating (63.71)				
House Challengers				
Personal meeting	+9.5	(1.9)	.03	+0.23
Group meeting	−2.4	(2.5)	.02	−0.05
Staff	+7.1	(2.7)	.02	+0.14
Mailing	+2.6	(0.9)	.13	+0.34
Radio	+1.6	(1.0)	.11	+0.18
Television	+1.8	(0.8)	.20	+0.36
Newspaper	−0.6	(0.8)	.24	−0.14
Indirect	+0.3	(1.2)	.08	+0.02
Intercept (50.05) + Total Effect (+1.13) = Average Rating (51.18)				
Senate Incumbents				
Personal meeting	+7.7	(3.4)	.07	+0.54
Group meeting	+5.0	(3.2)	.08	+0.40
Staff	+5.7	(3.7)	.05	+0.28
Mailing	+1.8	(1.7)	.41	+0.74
Radio	+2.4	(1.8)	.40	+0.96
Television	+3.7	(1.9)	.72	+2.66
Newspaper	−1.4	(1.8)	.60	−0.84
Intercept (55.42) + Total Effect (+4.74) = Average Rating (60.16)				
Senate Challengers				
Personal meeting	+16.4	(3.8)	.04	+0.66
Group meeting	+8.7	(4.1)	.03	+0.26
Staff	+7.2	(4.2)	.03	+0.22
Mailing	+0.8	(1.7)	.22	+0.18
Radio	+2.8	(1.6)	.30	+0.84
Television	+6.5	(1.6)	.61	+3.96
Newspaper	−3.3	(1.6)	.49	−1.62
Intercept (51.30) + Total Effect (+4.50) = Average Rating (55.80)				

Source: 1978 Election Study, Center for Political Studies, University of Michigan.

Table 4. Percentage Voting for Incumbent in House and Senate Contests by Evaluation of Incumbent and Exposure to Challenger

Exposure to Challenger	Evaluation of Incumbent: Positive	Evaluation of Incumbent: Neutral or Negative	Tau	(Sig.)
House				
Yes	84	29	.43	(.001)
No	93	65	.24	(.001)
Senate				
Yes	78	22	.59	(.001)
No	83	72	.15	(N.S.)

Source: 1978 Election Study, Center for Political Studies, University of Michigan.

bent reported having had some contact with the challenger.

The greater visibility of Senate challengers is not, however, the only difference between House and Senate elections. In addition, House and Senate candidates seem to be evaluated according to different criteria. Evaluations of House candidates were based largely on contacts between constituents and their representatives, which were overwhelmingly positive in nature. Contacts with senators did not have as large a net positive impact, suggesting that voters were less dependent on these contacts for information about the incumbent. Party affiliation and ideology had a greater impact on evaluations of Senate candidates than on evaluations of House candidates. The result of this is shown in Table 5. While ideological proximity to the incumbent had a negligible impact on candidate choice in House elections, it had a substantial impact on candidate choice in Senate elections among two groups of voters: independents and supporters of the challenger's party. The ability of Senate incumbents to draw support from independents and voters of the opposing party depended largely on their ideological position. Whereas House incumbents were favored by an overwhelming 66 percent of voters whose ideology was opposite to their own, Senate incumbents received only 30 percent of the vote among this group.
be held accountable on the basis of their overall voting record than representatives. It does appear that constituents were more likely to know something about their senator's voting record than about their representative's voting record. Whereas 51 percent of respondents were able to place their Senate incumbent on a seven-point liberal-conservative scale, only 37 percent were able to place their House incumbent on the same scale. In addition, perceptions of senators' positions appear to have been somewhat more accurate than perceptions of representatives' positions.[2] We can only specu-

Table 5. Percentage Voting for Incumbent in House and Senate Contests by Party Identification and Ideology

Party Identification	Ideological Proximity to Incumbent: Closest	Medium	Farthest	Tau	(Sig.)
House					
Same party	96	94	100	–.00	(N.S.)
Independent	87	81	69	.13	(.05)
Other party	52	46	40	.11	(N.S.)
Senate					
Same party	87	90	*	–.02	(N.S.)
Independent	83	62	39	.33	(.001)
Other party	39	15	7	.30	(.01)

Source: 1978 Election Study, Center for Political Studies, University of Michigan.

*Fewer than 10 cases.

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[2] The correlation (Tau) between ideological proximity and perceived ideological distance was .26 for Senate incumbents and .15 for House incumbents.

late that this may be a function of the information available to constituents about their senators and representatives. With greater control over the information constituents receive about themselves, House members may stress noncontroversial activities such as casework and constituency service, rather than positions on controversial national issues. These findings may also reflect greater involvement of ideological or single-issue interest groups in Senate campaigns. Whatever the explanation, the result is a significant difference in the extent of ideological voting in Senate and House elections.

Even if House challengers were provided with resources which would enable them to reach many more voters with their message, most House incumbents would probably remain secure in their positions because the large majority of their constituents are very satisfied with their performance.[3] When asked to rate the job performance of their own representative, 72 percent of respondents expressed an opinion, and 65 percent of those with an opinion gave a rating of "very good" or

Discussion

The evidence available from the 1978 election study does not allow us to answer the question of why senators were more likely to "good," while 31 percent gave a rating of "fair" and only 5 percent gave a rating of "poor" or "very poor." In sharp contrast, among the 80 percent of respondents expressing an opinion about the performance of Congress, only 20 percent gave a rating of "very good" or "good," while 52 percent gave Congress a "fair" rating and 28 percent rated its performance as "poor" or "very poor." These findings lend strong quantitative backing to Fenno's conclusion (1975), based on his observation of ten House members in their districts, that voters love their congressman but dislike Congress. Although the job performance question was not asked about Senate incumbents, comparison of the feeling thermometer ratings of senators and representatives indicates that voters do not love their senator quite as much as they love their representative.

It is often suggested that members of Congress can use noncontroversial activities such as casework and constituency service to earn political support from the voters and thereby gain freedom of action on important national issues. Voters, according to this view, are tolerant about members' positions on national issues as long as they answer their mail, act as ombudsmen, and bring federal projects into the district. The findings reported here suggest, however, that voters are more tolerant about their representative's voting record than about their senator's voting record. After the 1976 and 1978 elections, many senators are probably looking over their shoulders as they cast their votes.

Appendix

The measure of ideological proximity was constructed by comparing senators' and representatives' ADA ratings with respondents' positions on the seven-point liberal-conservative scale (1 = most liberal, 7 = most conservative). ADA ratings were used because of the well-established liberal identification of this organization and because of the wide variety of issues on which the rating was based. ADA ratings also correlate very highly with ratings of other ideological groups. Ideological proximity to the incumbent was calculated as follows (1 = closest to incumbent, 3 = farthest from incumbent):

ADA Rating	Respondent's Position	Proximity Score
Senate		
0–19	1–3	3
0–19	4	2
0–19	5–7	1
20–39	1–3	2
20–39	4–7	1
50–59	1–4	1
50–59	5–7	2
60+	1–3	1
60+	4	2
60+	5–7	3
House		
0–39	1–3	3
0–39	4	2
0–39	5–7	1
40–59	1–3	2
40–59	4	1
40–59	5–7	2
60+	1–3	1
60+	4	2
60+	5–7	3

Note: No running Senate incumbent had an ADA rating between 40 and 49.

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[3] Two recent studies of campaign spending in congressional elections have concluded that higher spending by challengers significantly improves their performance while spending by incumbents has little effect (Glantz, Abramowitz, and Burkart, 1977; Jacobson, 1978).

This procedure resulted in similar distributions on the ideological proximity scale for voters in House and Senate elections.

Respondents who did not have an opinion about a candidate were assigned to 50 degrees on the feeling thermometer. The relative position of the two candidates on the feeling thermometer was a very reliable predictor of the vote.

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The American Voter in Congressional Elections

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The article analyzes congressional elections research following from the 1978 National Election Study. In a field where basic information was lacking, the study constitutes a major data collection effort. Results should be taken as tentative, with serious work on measurement and conceptualization remaining. Nevertheless, a number of important preliminary findings can be identified. Voters' evaluations of the congressional candidates, House and Senate, have a major influence on the vote, separate from incumbency and party and more important than presidential evaluations or other evaluations. While House incumbents receive the strongest positive support on a number of measures, there is little negative perception of any candidate in congressional contests. Finally, there are major differences found for Senate and House challengers, in voter recognition and information, but no major differences for Senate and House incumbents. House challengers stand apart from all other candidates in their degree of visibility and contact with voters. The article discusses the implications of these findings and indicates priorities for future research.

This article examines the state of congressional elections research following the 1978 National Election Study.[1] Before the study, scholars could agree that basic information on the subject was lacking. Within a few years' time, we find great productivity and further work under way. We need recognition of the advance, caution as to the provisional state of the present findings, and some thought about future directions. We need, in short, an analytical accounting of recent congressional elections research, its activity, and implications.[2]

Past Research

In the literature on elections, the "American voter" is a presidential voter. Most of what we know about party attachment, issue voting, efficacy or participation has developed primarily from that atypical contest. Political scientists, like the voting population as a whole, have shown neither great interest in, nor much attention to, races other than the presidential one. So while the American public elects both a president and members of Congress to guide the policies of the nation, only one-half of that process has been studied intensively. Writers have examined short-term presidential and economic effects on congressional voting without measuring short-term congressional effects. Money is spent, and campaign spending laws are reformed, without spenders or reformers knowing the impact of money on election results. Journalists, candidates, and campaign managers speak of the importance of this or that issue or strategy or candidate image, with no data on whether what was transmitted from the campaign was ever received by the voters.

Some work, of course, proceeded in the study of congressional elections. A major thrust of this research sought to identify critical variables affecting the vote: party identification, incumbency status, candidate and issue evaluations, as well as influences of presidential voting. Work on the effects of campaign spending was begun (Glantz et al., 1976; Jacobson, 1978, 1980). While House contests received the predominant share of attention, some work compared Senate results to other statewide voting (Wright, 1974; Hinckley et al., 1974). In addition, the peculiar safeness of House incumbents was examined at length and various explanations developed (Erikson, 1971; Mayhew, 1974b; Ferejohn, 1977; Cover, 1977). Studies pointed to a complex of factors explaining incumbents' success including visibility, office support, and a general positive reputation. Within this complex, however, two factors have been given different explanatory emphasis; those of *voter information* and *incumbent activity*. In some explanations, incumbency, like party, supplies an easy voting cue in the low-information context of congressional voting (Miller and Stokes, 1962; Kostroski, 1973; Hinckley et al., 1974; Nelson, 1978). To be recognized, even in vague and general terms when the challenger is not so recognized, is a strong electoral advantage. Party and incumbency may therefore be interconnected: the decline of one bound together with the rise in importance of the other. Other explanations emphasize the incumbents' activity. Incumbents run interference for constituents with the bureaucracy, claim credit for federal projects in their districts, and provide other popular and noncontroversial constituency services (Mayhew, 1974a; Fiorina, 1977). Even independently of these activities, incumbents work hard to create a positive reputation for themselves in their "home styles" and district activities (Fenno, 1978). Campaigning, in other words, can be done on the job, between elections (Jones, 1966, p. 29). This difference in emphasis can be summarized as follows. In some explanations, incumbents must do something right to win elections; in others, incumbents might well be reelected whatever activities (within reason) they pursue.

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[1] The survey, directed primarily to voting for Congress, was supported by the National Science Foundation and conducted by the University of Michigan's Center for Political Studies.

[2] The main "data" for the analysis are the first 16 papers resulting from the study plus participant-observation in the conferences and committees of the past three years. Papers were presented at the 1979 American Political Science Association meetings and the Houston-Rice Conference on Congressional Elections, January 1980.

Nevertheless, as these writers themselves agreed in a Rochester conference in 1977, basic survey information was lacking. Most of the biennial National Election Studies asked few questions on Congress beyond the vote. The questions asked for the House in 1958 and the Senate in 1974 were not repeated over time or replicated for each other. Voter information on congressional candidates could not be measured beyond the memory-testing "recall" question,[3] nor could sources of information be studied. What was perceived or liked about House incumbents could not be analyzed nor compared with the Senate where incumbents were increasingly losing reelections. In short, some of the major hypotheses circulating in the literature could not be tested.

Seen in this context, the major purpose of the 1978 study was to *supply such basic information.* In the study of congressional elections, this survey constitutes a major data collection stage. While the survey was designed as a national sample, the congressional district was designated as the primary sampling unit. Accordingly, congressional candidates could be identified with their opponents, district characteristics, electoral situation, and campaign spending. Candidate names were supplied for a new name-recognition question, simulating the kind of cognitive activity demanded in the voting booth. Voters were asked, with the names of the candidates provided, if they recognized the names and could evaluate them positively or negatively, rating them from 0 to 100 degrees on a thermometer scale. A battery of questions probed what voters knew, saw as important, and liked about incumbents. Another battery asked about the contacts between respondents and candidates: by mailings, personal meetings, mass media, or other sources. Questions were matched for Senate and House to facilitate cross-chamber comparisons, and a large amount of contextual data—on the district, the campaigns, and the candidates—was compiled.

Advances and Limits

The 1978 congressional elections data were analyzed in papers presented at the 1979 American Political Science Association meeting and the 1980 Conference on Congressional Elections at the University of Houston and Rice University. Clearly, the results so far need to be taken as tentative and preliminary. We find some first looks at the data and a number of cross-tabulations, but as yet no analysis in depth. More seriously, the utility of the various items has not been subjected to close examination. For example, the "contact" questions have been used in a number of papers, with contacts typically correlated with the extent of positive candidate evaluations. (For a list of these questions, see Mann and Wolfinger, Table 11 [1980, p. 627], and Abramowitz, Table 3 [1980, p. 637].) These questions, however, may tap very different dimensions of electoral activity: the candidate's activity as in contact through mailing; the voter's participation and interest as in personal contact; and media reporting as contact. Since some items may measure distinct activities and others may best be combined to describe one activity, further analysis is necessary before we know how they should be utilized—what "contact," in fact, is being measured. The thermometer ratings have also been employed widely, and very differently, and need much more measurement attention. (For the distribution of ratings, see Table 2 below.) The whole problem of incumbency needs more comprehensive and refined analysis. Incumbents are liked, voted for, recognized, rated well in job performance, and, when issues can be identified by respondents, seen as agreeing generally with the respondents' views on issues. But we do not know how much liking is knowing, how much issue agreement is merely liking (as opposed to liking because of issue agreement), and how much of all this is the result of not knowing the challenger. This point will be elaborated further in a later section.

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[3] The question asks "Do you happen to remember the names of the candidates for Congress—that is, for the House of Representatives in Washington—that ran in the district this November?" It is asked in a post-election survey, typically conducted through December and January following the election.

All this is fine for a first reporting of results. However, since these papers will achieve wider visibility, because of the nature of the project, than perhaps they would otherwise, it is important to keep in mind the preliminary status of the inquiry. The sheer number of papers being reported should not discourage others from the work left to do.

Recognizing these cautions, one still can see the main lines of a collective advance. The following already can be cited as major results of the study.

Voters' evaluations of the congressional candidates, House and Senate, have a major influence on the vote, separate from incumbency and party and more important than presidential evaluations or other evaluations (see Mann and Wolfinger, 1980; Hinckley, 1980; Parker, 1980; Ragsdale, 1980; Jacobson, 1980; Kuklinski and West, 1980). Although this influence is assumed by some democratic theory, it has not been highlighted in the past voting behavior literature, and it is now clearly pinned down in importance. The results hold whether measured by open-ended questions or the thermometer ratings. The open-ended likes and dislikes questions have become critical to presidential voting studies, measuring short-term forces—of parties, issues, and candidates—in a campaign. The present finding suggests they could be similarly important in analyzing congressional contests.

This finding also questions some of the past interpretations of midterm elections (see especially Ragsdale, 1980; Kuklinski and West, 1980). Several past studies are often cited to show the presidential influence on the congressional vote (see, for example, Tufte, 1975; Kernell, 1971; Piereson, 1975). However, the past studies do not control for congressional variables of importance. Neither incumbency nor congressional candidate evaluations are controlled for, while party identification is only partially controlled for. When the congressional variables are added, as in the 1978 studies, presidential influence is considerably reduced in importance.

There are major differences in voting for Senate and House challengers—in recognition, information, campaign behavior, and expenditures—but no major differences for Senate and House incumbents (see Abramowitz, 1980; Hinckley, 1980; Uslaner, 1980; Jacobson, 1980; Goldenberg and Traugott, 1980). House challengers stand apart in comparison to other House candidates and all Senate candidates in their degree of visibility and contact with voters. Table 1 summarizes the situation.[4] While most voters could recognize and rate the incumbents and the Senate challengers, less than half could recognize and rate the House challengers. When one compares challengers with other non-incumbents, one finds Senate challengers to be like the other candidates in degree of recognition and House challengers not. While 78 and 70 percent, respectively, recognize House Democratic and House Republican non-incumbents, only 44 percent recognize the House challenger. The same pattern is seen in the contact questions. In contrast to the frequencies for other House candidates and for all Senate candidates, less than half of the voters said that they had any contact with, or learned anything about, the challenger.[5] Even controlling for party identification, challenger contact and recognition remain low. Only about half of the voters of the challenger's party could recognize and rate the challenger in the contest they voted for.

At minimum, these results suggest that an understanding of congressional voting cannot be generalized from the House alone or from arguments about House incumbents. This finding, however, has wider implications. In the Senate, where voters choose between two candidates they recognize and have had contact with, minimum conditions for competition between candidates are present. In the House by contrast, for any of a number of reasons which need to be investigated more fully, these conditions are not present.

Another major finding confirms the positive support for House incumbents. They receive the highest thermometer ratings of any candidates and mostly get "good" or "very good" job ratings. Moreover, their support is not attributable to their recognition alone. Respon-

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[4] The table and subsequent discussion are drawn from Hinckley, 1980.

[5] Since the Senate, unlike the House, is proportioned by states and not by population, a disproportionate number of Senate respondents in the sample will fall in states of large population: in 1978, the states of Michigan, Illinois, and Texas. However, dichotomizing the Senate respondents into those from the three largest states in population and those from all other states showed no important differences in results. In both recognition and contact, Senate challengers in both dichotomous groupings remained sharply distinct from House challengers and similar to the other Senate candidates. For details, see Hinckley, 1980.

Table 1. Name Recognition and Contact in the Senate and House[a]

	Percent of Voters Who Can Recognize and Rate Candidates[b]				Percent of Voters Who Had Contact with Candidates			
	House		Senate		House		Senate	
	%	N	%	N	%	N	%	N
Incumbents	92	(757)	95	(395)	94	(757)	97	(395)
Challengers	44	(757)	85	(395)	46	(757)	85	(395)
Non-incumbents (D)	78	(116)	92	(150)	81	(116)	95	(150)
Non-incumbents (R)	70	(116)	83	(150)	72	(116)	91	(150)

Source: 1978 National Election Study, Center for Political Studies, University of Michigan; data computed by the author.

[a] The results are based on voters in contested elections, with two major party candidates in the contest. The term "incumbent" refers only to those running for reelection and "non-incumbent" refers only to those non-incumbents running against other non-incumbents.

[b] A very small number of respondents who said, yes, they could recognize the candidates then said, no, they could not rate them. The measure reported here is based on those who could both recognize and rate.

dents can give reasons for liking incumbents—citing experience, personal qualities, and district attention—in the open-ended questions (see Parker). Yet we need to be cautious in identifying the basis of this apparently pervasive support. As Samuel Patterson commented at the Houston-Rice conference:

> Psychological analyses of voting choice tend to reach tautology. Many folks in the 1978 CPS sample, in the compass of a short interview with a stranger, say that their congressman is popular with them, they like him and rate him warmly on the feeling thermometer, they prefer him and things about him, and they say they voted for him. Are these different things, or all part of a more general phenomenon called support for the congressman?

Comparing the numbers of respondents in the list below shows clearly the difference in general and specific support. Of the 2304 respondents in the sample,

1760 said they had some contact with their representative;

1406 felt their representative could be at least somewhat helpful if they had a problem the representative could do something about;

1058 gave representatives "good" or "very good" job ratings;

650 said they agreed at least somewhat with their representative's voting in Congress (a large majority, 1335, could not answer the question);

458 said they could remember something the representative had done while in Congress for the district or its people;

293 preferred one of the candidates because of an issue in the campaign in their district (664 could identify such an issue of at least some importance to them);

221 could remember a particular bill the representative voted on, and 151 agreed with the representative's vote.

Unfortunately, these questions were not asked for the Senate, with the exception of the "contact" question. Therefore we cannot compare support for House and Senate incumbents or relate it to their very different reelection rates. The specific content for liking and the ways information is translated into liking remain to be identified. Attentive people in the district might agree with their representative's policy positions and remember things the representative has done for the district. These perceptions might be diffused—through media content, campaigning, and conversation—to much more widely held general impressions. Alternatively, some of the general support may have no basis, either direct or derivative, in specific perceptions. Some people may rate their representatives as doing a good job because they expect them to do so and have sought and heard nothing to the contrary. No challenge to the general support has occurred. Survey data, of course, are limited in their ability to separate these alternatives. Nevertheless, repeating the questions for Senate incumbents should help to isolate which of these various measures of support, if any, have effects upon voting behavior.

One final result can be cited, implicit in several studies. *There is little negative perception of any congressional candidates, whether Senate or House, incumbents or challengers.* Moreover, there is little negative perception even from the highly informed campaign contributors (see Powell). Table 2 shows the similarity of ratings for House and Senate incumbents and the concentration for all candidates within the "warm" range of the thermometer scale (between 50 and 90 degrees). More than 90 percent of voters in contested elections gave House candidates positive or neutral ratings and approximately 80 percent gave Senate candidates these ratings. Congressional elections do not seem to be a matter of tossing rascals out or screening would-be rascals from getting in. Accordingly, some issue voting and other choice models might be misdirected. For a large number of House voters, it was seen above, essentially no choice is provided: one candidate is known and the opponent is not known. In many other contests, in both the Senate and House, the choice involves either two positive or one positive and one neutral perception. Hence, to analyze such contests we may need to identify the dynamics of choice in a generally positive situation: for example, with reference to the kinds of information received, sources of information, and campaign spending.

Consider, for example, the following two "job application" models, where voters are screening candidates for a job in Congress. One model assumes that the job demands specific characteristics and that applicants range considerably in these characteristics. Voters can declare Candidate X as unsuitable or Candidate Y as more suitable than others. The second model assumes that the job is less demanding of specific characteristics or that the candidates are all qualified by the time they reach this particular stage of the hiring process. Who, we might ask, will be hired in these circumstances—the candidate available at the right time, the one interviewed first, the one already making a positive impression from some other contacts? Students of voting may need to ask when and under what circumstances either of these processes will be followed. Both models describe a selection process, but they may follow from different circumstances, lead to different predictions of success, and produce different performances in office.

The Incumbency Advantage: An Illustration. We can use the problem of incumbency to illustrate the distance covered and the work ahead. The incumbency advantage in congressional elections was a major focus of past research and was emphasized in the 1978 study. The results illustrate in sharpest form the nature of the progress, the conceptual and methodological work remaining, and the implications for American elections. In asking why and under what conditions incumbents are reelected, we ask about the link between government and voters and about the voting

Table 2. Thermometer Ratings for Senate and House, Incumbents and Challengers*

Degrees on Thermometer	Percent of Voters Rating: Senate Incumbents	House Incumbents	Senate Challengers	House Challengers	(House Challengers II)
100	10	15	5	1	2
90–99	0	1	0	0	1
80–89	19	22	12	3	7
70–79	17	19	15	5	12
60–69	15	16	15	8	17
50–59	20	18	33	74	41
40–49	7	3	9	4	8
30–39	4	2	4	2	4
20–29	0	0	0	0	0
10–19	4	1	4	2	3
0–9	5	2	4	2	4
Total	(101%)	(99%)	(101%)	(101%)	(99%)
N of Cases	395	757	395	757	334
Mean Degrees	61.3	68.9	56.5	51.3	52.9
Standard Deviation	25.1	21.7	21.6	12.6	18.8

Source: 1978 National Election Study, Center for Political Studies, University of Michigan; data computed by the author.

*Based on scoring those who cannot recognize and rate as 50 degrees, for the first four columns of the table. Only a small number of voters are not able to recognize or rate the Senate candidates or the House incumbent. The final column, labelled House challengers II, shows the distribution for those who can recognize and rate the challengers.

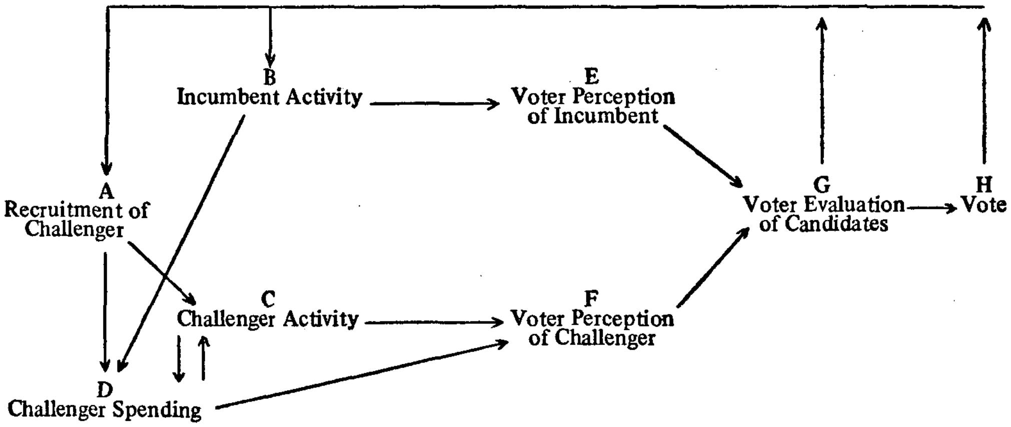
decision that says, in effect, government should continue as it was before.

Past explanation of the incumbency advantage, primarily focused on the House, could not be tested. We did not know what activities were perceived and liked by voters, could not measure visibility beyond the test of name recall, and could not compare either activity or visibility to Senate contests where incumbents were losing reelections. The 1978 results take us some steps further. Incumbents are known, contacted, and evaluated positively, and these factors are significantly related to the vote. But Senate incumbents are also known, contacted, and evaluated positively. Little difference is seen in the job expectations voters hold about the two offices. The few differences observable between House and Senate incumbents make no difference to the vote. It is with the challengers, rather, where the critical differences appear. Compared to Senate challengers or open seat candidates, House challengers stand apart in low visibility and contact, and a preponderance of neutral ratings. Research attention, then, might turn from incumbents—and what they do right—to challengers and what they do not do: to their recruitment, spending, and activities facilitating their election.

Given the earlier cautions, the argument *must* be considered tentative and subject to further testing. Measures need to be refined, and the data themselves replicated for other election years. One study indicates that the House challengers in the 1978 sample were unusually weak compared to past years—even for challengers (see Jacobson). At the same time, we cannot disregard the positive support shown for incumbents nor their success in getting across to voters. Models need to be developed to deal with what appears to be a highly intercorrelated and reinforcing process. However the subject is pursued, several additional questions must be posed and answered.

We can take the diagram in Figure 1 as an outline for the discussion. Earlier studies diverged in emphasizing candidate activity or voter information: we should now add the challengers' recruitment as a third explanatory component. We should also pay attention to challengers more generally by separating the effects of the two candidates in the race. One is not, except at the final voting stage, the necessary obverse of the other. Finally, we can focus on the individual voters—and their perceptions and behavior—taking recruitment and candidate activity as the political context influencing their responses. Party identification, with its well-known effects upon voting, can be assumed as controlled for. Note that the many possible interactions among these components are not included: these can be drawn or erased as specific models and hypotheses are developed. The diagram offers a general overview of the process only and the major components. As such, *it should clarify the kind of questions* to be asked and hypotheses to be developed and provide a general basis for discussion.

What are the independent effects of incumbent and challenger activity on voter evalua-


B
Incumbent Activity
E
Voter Perception of Incumbent
A
Recruitment of Challenger
G
Voter Evaluation of Candidates
H
Vote
C
Challenger Activity
F
Voter Perception of Challenger
D
Challenger Spending

Source: Diagram drawn by the author.

Figure 1. The Incumbency Complex: Recruitment, Candidate Activity, and Voter Information

tions? In the House case, how much of the results are explained by what incumbents do right (B to E to G) or what the challengers do not do (C to F to G)? How can we cut the reinforcing circle, at least analytically, between candidate recruitment, candidate activity, and the voters' receiving and evaluating of information? It might be argued, for example, that House incumbents, by being liked, discourage strong challengers from entering the race (B through to G to A). Voters respond positively to the incumbent's activity and the politically astute would-be challenger responds to the public support. Alternatively, incumbent activity may affect recruitment directly (B to A). Thus one's home style and activity in office are communications directed not so much to the voters as to potential challengers and their supporters. Voters become less important. In either case, once an incumbent is perceived as vulnerable or a strong challenger decides to enter the race, the effects of activity and visibility follow. For another alternative, incumbent activity may lead to positive perceptions by voters (B through to G) no matter what challengers are selected or what activities they pursue.

How important is money in the above process and what, in particular, are its independent effects? The 1978 spending data still await analysis with the new measures provided by the survey. Past studies show that campaign spending is important for the challenger, not the incumbent (Jacobson, 1978; Glantz et al., 1976). We should expect spending to affect challenger activity as well as the voters' perceptions that there is indeed a challenger in the race (lines D to C and D to F). But incumbent activity and voter evaluations can also affect that spending, as can recruitment, by constraining the amount of money available.

Are there several points or one critical point at which the circle can be cut? For example, controlling for recruitment, does the kind of activity, or merely the amount of activity, engaged in make a difference? Within what range and to what extent does the challengers' activity affect the vote (lines C through to H)? Despite the time and money spent on the assumption that it does, we do not know the answer to this question. Goldenberg and Traugott conclude from other data that a candidate's advantage in use of mass media and in money spent on advertising significantly affect the vote, independently of party and incumbency status. Voters, they suggest, are gaining something from media and advertising beyond familiarity with the candidate's name. This kind of work could be extended with data from the 1978 study.

We can turn to the many possible interactions not drawn in the diagram. What are the interactive effects produced by a campaign and how do these affect voters' information and evaluation? Consider the addition of reciprocal lines drawn between E and F. Table 2 shows Senate incumbents' ratings slightly lower than House incumbents. That may be the result of the representatives' more successful communication with voters *or* the result of Senate challengers' campaigns: reducing incumbent popularity either by reporting unfavorable information or by forcing voters to make comparative evaluations. By the same argument, House incumbents' activity may be so successful that it persuades voters that they do not need to make comparative evaluations. Challengers do not even need to be perceived. Consider also the addition of lines from E and from F back to both B and C. In some races, voters may be fairly passive recipients of information provided and not provided; in others, there may be a more active search for information. A number of other interactions could also be proposed.

Can we identify types of races by candidate recruitment, campaign activity and voters' perceptions: in effect, breaking down Figure 1 into separate diagrams? The results this far clearly indicate the need for such classification and subdivision. In some races and not in others two candidates are perceived by the voters. House races produce "serious" or "token" challenges. In some races, national or local issues may be important, but these need to be isolated from the overall positive support for incumbents. There may or may not be an active search for information by voters and different kinds of screening for that job in Congress. Identifying the range of choice processes and choice situations now subsumed under the label "voting" requires some initial classification.

These questions have obvious implications beyond the particular problems identified in the present research. They ask about the role of elites in recruiting candidates, about the activities in office necessary for reelection, and about what the American public expects and utilizes as sufficient information to make a voting choice. As such, they extend beyond the House and Senate and beyond incumbents to all elections. While some of these questions will be easier to operationalize than others, many can now be addressed with the 1978 study.

Future Research Priorities

So what of the future of congressional elections research, given the advances and limits identified? Even this brief review suggests a number of priorities.

Clearly, part of the work of the future must be to mine the present resources. Before too many articles go forward using a measure or two, we need some very serious work on the measures we are using. Designs are needed that can deal with complex and reinforcing processes, distinguish types of congressional voting, develop classifications, and isolate and measure independent effects. At the same time, the unexploited resources of the study need attention. Campaign spending has been only superficially dealt with and the rest of the contextual data remain untouched. Campaign materials have been collected and coded, although these additional data have not yet been released. These materials include, among other components, the kind of activity engaged in by candidates and the themes of the campaign: references, for example, to party, issues, personal characteristics and experience, or the opponent. All of these contextual data need to be integrated with the rest of the analysis.

Equally high priority should be given to developing a time series for the items found important in 1978. The thermometer ratings, the new name recognition measure, the open-ended likes and dislikes question, voters' sources of information about candidates—again and again are found useful in these papers. The congressional district sampling base and the coding by kind of candidate and kind of race are invaluable. One reason to repeat the questions over time is that the questions themselves have great value. A second reason is that the years themselves can become important variables. Understanding the impact on voting of national issues, presidential popularity, or events in the world requires time series data. A third reason is indicated by the difficulties intrinsic to any survey design. Questions have been raised about the idiosyncracies of the 1978 sample (see Jacobson; see also Mann and Wolfinger, this issue). These difficulties can be expected with any national population sample, since the constitutional framers in their wisdom did not design American elections to accommodate survey research. There are many problems, too, with a presidential election sample, but we have managed to use it to learn about presidential voting. Nevertheless, given these difficulties, it becomes all the more important to continue the time series past the vagaries of any one sample.

In designing a time series, it is worth remembering that congressional elections are held every *two* years, not every four years. Americans vote every second year under the Constitution. Surveys necessarily restrict the number of questions that can be asked in an interview schedule: pursuing some lines of inquiry precludes others. So what should be the place and importance of congressional survey questions in a presidential election year? If congressional elections are seen merely as a curious variant in the mainstream of voting studies, by definition presidential, then a quadrennial time series may be sufficient. Congressional questions could be reserved for off-year elections. If they are seen rather as the more typical voting activity engaged in by Americans, against which the presidential is the variant, then all researchers can learn from these questions. New evidence from 1978 questions some of the past presidential coattail studies. How much greater future progress on the subject could be if we had data on congressional evaluations in a presidential year and off-year sequence, and could repeat the question across time and across different presidential contests.

It is also worth remembering that Congress is not unicameral and that "House" is not a synonym for Congress. The recent findings highlight the importance of maintaining and increasing Senate and House comparisons. If a question is considered important to congressional elections, it is worth asking for both the Senate and House: indeed, the similarities or differences in the answer may be the most valuable result. While Senate incumbents are defeated more frequently than House incumbents, they engage in similar kinds of contact, are rated almost as positively, and are not expected, in comparison to representatives, to act any differently in Congress. Arguments about the electoral importance of contact, job expectations, or negative versus positive ratings must be revised accordingly. Other questions, we saw, could not be given the same scrutiny because of the absence of Senate comparisons. The open-ended questions regarding likes and dislikes, on the basis of the findings thus far, should be as important to congressional voting studies as they have been for presidential voting. Yet no national election study has ever asked—in the same survey—these questions for both Senate and House.

Some new questions also are suggested by the recent results. The 1978 study asked voters why they liked candidates, in the process making two assumptions: that there was information as content for liking; and that the content came from the activity of the candidates themselves, whether campaigning, constituency service, or roll-call voting in Congress. Future studies might well target these assumptions. What is sufficient information to reach a "liking" decision? Under what conditions is a search for further information initiated or ended? What, in other words, do people see as sufficient information for a voting choice? There may be, as suggested earlier, different kinds of information processing engaged in depending on the kind of contest and candidates in the race (controlling as necessary for the kind of voter). One suggestion, then, is for a battery of questions on the choice process analogous to the battery on liking in the 1978 survey. Theories of choice and information processing, developed from a range of studies not limited to the political, should be helpful in directing these questions.

By collecting contextual data, we acknowledge that there is a critical political environment—or context—within which individual voting occurs and which needs to be measured independently of voter perceptions. Several points in that environment are specifically targeted by the 1978 results. The weakness of House challengers leads directly to questions about recruitment which in turn leads to the "recruiters" and their activity. Who is it incumbents must "keep in touch" with and whom is the home style presented for? How important are party and non-party groups in the decisions of challengers to seek office? What is the role of money, endorsements, or other activity in encouraging and discouraging these decisions? Endorsements, themselves, may be seen as points in a spectrum of communication by which political activists communicate to other political activists (as well as to voters) about who is and who is not to be encouraged for office. Thought might be given to feasible ways of collecting information about these activities, whether by elite interviews, measures of party and interest group activity, or other materials. Elite interviews, in particular, could have a wide variety of uses, many already indicated at points in this discussion.

Cautions are necessary, too, in utilizing such data Care needs to be taken in the kinds of questions asked. With a national sample of voters—not races—contextual data cannot measure any particular set of contests, but data can measure the variability of influences affecting voters in the sample. For example, data can measure those influences diagrammed in Figure 1 and it can help classify the kinds of contests and choices offered voters. Care is necessary also in interpreting results so as not to confuse contextual and individual-level measures. Campaign activity measured by elite interviews is different from campaign activity measured by the contact questions. What candidates do is not necessarily what voters perceive. The communication encoded by one is not necessarily decoded by the other; indeed, one value of utilizing the separate measures will be to see how much effective communication occurs. Designs have been constructed merging contextual and individual data (see, for example, Miller and Stokes, 1963; Patterson et al., 1975). Further work on such design, as well as some use of the contextual data already collected, would seem to be indicated.

This research marks a shift and broadening of attention from voting to elections. Seeing voters as screening candidates for a job in Congress, we ask about the kind of candidates who get to the interview, the questions they are asked, and the implications for past and subsequent job behavior. Like House incumbents, we move from Congress to the voters and back to Congress. This broader political perspective has value for all voting studies, but it is particularly appropriate as pursued in the congressional field. Students of Congress have already compiled rich "contextual data" through the years of studying congressional behavior. What had been missing was data on voters. The 1978 study begins to correct the omission while it suggests the possibilities for combining both kinds of information.

The advance in congressional elections research can be marked by the course of this discussion. We have moved beyond identifying basic variables and speculating about House incumbents. We can identify new findings, see the variability across kinds of contests, question earlier hypotheses and raise others. Most critically, we can begin to see the amount of work ahead—in pursuing, beyond the presidential contest, the study of American elections.

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A Dynamic Model of Presidential Nomination Campaigns

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Recent campaigns have demonstrated the importance of dynamic elements in affecting the selection of presidential nominees. This paper develops a mathematical model to analyze these dynamics. The heart of the model is the assumed relationship between the ability to acquire resources and success in primaries and caucuses. The expenditure of resources leads to greater electoral success, while greater electoral success (in particular, exceeding expectations in a primary or caucus) leads to greater resource-gathering capabilities. A difference equation model of these relationships is proposed. I prove that any campaign of this form is necessarily unstable, which implies that most candidates will be "winnowed out" necessarily, while only a very few, but at least one candidate, will necessarily "have momentum." This result is true whether there are two or many contenders. However, I also argue that the larger the number of candidates, the stronger the dynamic forces, and thus the more rapid the "winnowing out" process.

Perhaps the single most distinctive characteristic of a U.S. presidential election is its length. Most democracies select their chief executives after a campaign of little more than a month or two, approximately the length of our general election campaigns. Our nomination campaigns, however, add an additional six months or so of formal proceedings, and nomination campaigns are newsworthy events for nearly a full year. This protracted duration of nomination campaigns, combined with the barrage of 30 primaries in 1976 and 37 in 1980, is reflected in the emphasis which media reports place on dynamic factors.[1] Questions such as who has "momentum" and who does not play a much more prominent role in news analyses of nomination campaigns that in commentaries about general election campaigns. These dynamics are covered not only in terms of primaries, votes, and delegates, but also in terms of such resources as money and Gallup poll standings.

To be sure, the shifting fortunes of the candidates have been important features of many recent general election campaigns for the presidency, such as Truman's last-minute surge in 1948, Nixon's inability to hold his early advantage in the polls in 1960, Humphrey's rise from disastrous early circumstances to make the 1968 election so close, and Carter's loss of his monumental lead early in 1976. It is equally as clear that such factors as personal abilities and issue concerns are important components of nomination campaigns.[2] Nonetheless, the collective wisdom of commentators and political scientists alike suggests that dynamic or time-related factors play a central role in our understanding of the nature of presidential nomination politics.

The purposes of this article are to isolate certain characteristics of this institution for selecting major party nominees, to propose a dynamic (time-dependent) model of these characteristics, and to study the properties of this model. Major questions addressed in this model include whether or not there is a stable equilibrium to the dynamics of the model, the role which resource acquisition plays in generating support for a candidate for the nomination and vice versa, and whether or not there are systematic differences between a two-candidate

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[1] In the model to follow, the referent is the presidential primary. The model, however, is compatible with the state caucus or convention procedures. Ordinarily, then, if I use the word "primary," it can be read as "some state delegate selection procedure." Exceptions will be noted where applicable, such as instances in which media coverage is important. In such cases, "primary" will refer to most primaries but to only a few caucuses (e.g., early ones like Iowa and perhaps some late ones).

[2] Obviously, contemporaneous media accounts and journalistic reports of elections after the fact discuss such matters. Within the discipline, the work of Orren and Schneider (1974), for example, present a strong case for the relevance of issues.

and a multi-candidate contest such as the Republican and Democratic contests in 1976. Since the basic result obtained in this article is the proof that there are no stable equilibria, a final question is how unstable is the model, i.e., how rapidly does the system "explode" relative to a nomination campaign of finite duration?

Basic Notions Underlying the Model

The heart of any model is its assumptions. While these assumptions will be specified in the next section, I should explain some of the most important ones briefly. The central relationship in the model is that between resources and success in particular primaries or caucuses. First, it is assumed that if a candidate invests greater resources in a given primary campaign, he or she will do better than if fewer were invested (and, consequently, opponents will do worse), *ceteris paribus.* I presume that this relationship is relatively unproblematic, but this relationship is but half. I also assume that the better candidates do in a given primary, the more resources they can gather for use in subsequent competitions, *ceteris paribus.* In short, greater resources lead to greater electoral success, which in turn lead to the ability to gather resources at a greater rate. All candidates, of course, attempt to generate this positive spiral. While some do (and presumably therefore have what is commonly called "momentum"), others do not. Those who see a negative spiral (call it "decay") are presumably those who are "winnowed out" of the campaign. The proof of instability, therefore, is the demonstration that at least one candidate will have momentum, while at least one (and usually many) will be winnowed out, barring the fortuitous circumstance of the campaign resting at an unstable equilibrium without perturbation.

A candidate's resources can be viewed broadly. Some (e.g., general ideology, eloquence, etc.) are relatively constant over time and will be included as a constant term with respect to time. Some resources may vary unpredictably and will be modeled as a stochastic term. Still others may vary but in ways predictable in advance (e.g., Reagan's appeal as a conservative in Texas, Ford's as a more moderate in Massachusetts). Other resources, however, will be time-variable. These resources are, of course, of central concern here and include money, media attention, the attraction of campaign volunteers, and perhaps even general popular support (as measured, for example, in Gallup polls).

What, then, of success? Ultimately (or, more accurately, penultimately), success is measured by gathering the support of a majority of convention delegates. By and large, the support of most delegates is now won or lost in state primaries or caucuses (and, by implication, convention delegates are no longer "free agents" at the convention).[3] Clearly, winning more votes and/or delegates in a primary is more of a success than winning fewer. Equally clearly, however, the results of individual primaries must be integrated into the larger goal of winning the nomination. In the next section, I will propose a measure, called the "competitive standing" of a candidate at a given time. While I will not use its particular properties, it seems reasonable to base such a measure on delegates currently supporting the candidate, those supporting opponents, and those who form the pool of available delegates (as yet unselected, uncommitted, etc.). However, unless one candidate has already won commitments from a majority of delegates, knowledge of just delegates won or lost is insufficient for assessing a candidate's chances. I presume that it is meaningful to speak of the probability of a candidate winning the nomination at any time. Indeed, for purposes of modeling, I assume that there is consensus over these probability statements, and that "momentum" or "decay" refer to "significant increases or decreases" in such a probabilistic assessment over time. While I will not rely on these probabilities, I assume that they can be formed. However such probabilistic assessments are made, they must rest on some measure of delegates.

Merely counting delegates at any given time is insufficient. In 1976, for example, it was reasonable to assume that Reagan would win all 167 California delegates in almost all circumstances short of his being mathematically eliminated already. That is, there were expectations about which candidate would win how many delegates, where, and thus when they would be won. Indeed, the "expectations game" is a major part of determining who won and who lost a primary. "Success," that is, lies in winning more votes and/or delegates in a primary than expected.

Donald Matthews, in discussing the role of the media in "winnowing" the list of nomination candidates to some smaller set, describes the process in very similar terms to those used here:

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[3] This scenario seems to underlie such dynamic models as in Brams (1978), Straffin (1977), and Collat et al. (1976). The central question in these models seems to be how delegates can maximize their influence. The model presented here is compared to these and related models in Aldrich (1980).

> More often than not, winning a presidential primary means doing better than expected; losing means disappointing expectations. The media find a winner in this curious contest by arriving at a rough consensus on how candidates should do and then measuring the vote and delegate outcomes by this rubbery yardstick. This "numbers game" is a big part of primary election reporting–how many votes and/or delegates must candidate "X" win before he can be said to have "won" (or "lost")? Without such "analysis" the raw facts and figures of presidential primaries would be meaningless to most Americans (Matthews, 1978, p. 63).

The assumptions here are nearly identical to Matthews' accounting. Beyond the small proportions of delegates won or lost in any individual primary, what is important is the comparison between what actually transpired and the "rough consensus" concerning the expectations about the outcome. I differ primarily by assuming that the "rubbery yardstick" can be modeled as made of stiffer stuff and that the candidates, as well as the media, play major roles in determining the consensus (see Aldrich, 1980, for explanation and evidence).

Expectations clearly precede the event itself. In practice, the shape of one primary campaign is not set in detail until after the immediately preceding primary or primaries. Thus, I assume that expectations are "forecasts" of likely results determined no later than shortly after the preceding primary. This assumption allows candidates to modify their campaign strategy after any or all earlier events, and thus potentially to modify the difference between actual and expected results. For example, Reagan initially offered his foreign policy themes in the Florida primary, in part because of the apparent failure of his welfare transfer plan in New Hampshire. I am assuming that expectations about Reagan's chances in Florida did not take his new policy initiative into account. (In fact, the foreign policy theme went virtually unnoticed nationally until weeks later.) For another example, a candidate could revise his resource allocations in light of expectations and prior results. It is assumed that this revised plan does not modify significantly the consensus of expectations. To illustrate, Ford (and the rough consensus) believed that he would win the North Carolina primary. Partly because of those expectations, his pollster, Robert Teeter, reports (Moore and Fraser, 1977, p. 44) that Ford and his staff chose not to invest as many resources in that primary as they had planned initially. Ford's loss there may be *explained* (in part at least) by that decision. However, the *effect* of the defeat, it is assumed, would be the same regardless of his choice of resource expenditure levels (at least barring a complete withdrawal; see the discussion below). That is, we can account for Ford's failure to meet expectations, but the fact that he failed to meet them, whatever the reasons, boosted Reagan's and hurt his chances at the nomination.

Deviations from expectations, then, yield changes in the competitive standings of the candidates and, hence, in the probability of their nomination. While I shall model these differences from expectations in terms of expected and actual delegate awards (via the measure of competitive standing), the model would be unaffected if I focused on, say, proportion of votes won instead of delegates, or perhaps varied this measure between votes and delegates as the details of the state's delegate selection procedures vary. Given the appropriate measure, the question focuses solely on actual as compared to expected results.

These elements are the integral parts of the dynamic model, and thus deserved special attention. Figure 1 presents a schematic diagram of this conception for the two-candidate case, by way of summary. I believe that most of the "story" told so far contains few surprises. It strikes me as consistent with the views of journalists (e.g., Witcover, 1977; White, 1969) and of political scientists (e.g., Matthews, 1978; Davis, 1967; Robinson, 1977). It also has a certain degree of face validity. For example, it seems consistent with Carter's "rise from obscurity," as well as the fortunes of Muskie and McGovern in 1972 and other instances. In Figure 2 I have plotted Carter's "competitive standing measure" (based only on the results of the primaries compared to that of his closest competitor), his Gallup poll standings among Democrats, the amount of money he was able to raise from individual contributors (i.e., excepting PAC contributions, money raised from sales, etc., and federal matching funds), and the proportion of *New York Times* stories about Democratic candidates that were concerned with Carter.[4] The strong coincidence of changes in delegate standings with increased flows of these "resources" suggests that it is plausible to model campaign dynamics, a task to which I now turn.[5]

[4] This figure is based on data reported in Aldrich (1980).

[5] This changing resource base is related to his success or failure in primaries in more detail in Aldrich (1980). Beniger (1976) illustrates this general pattern of "momentum" type dynamics for the 1936–1972 period.

The Assumptions and Notation of the Model

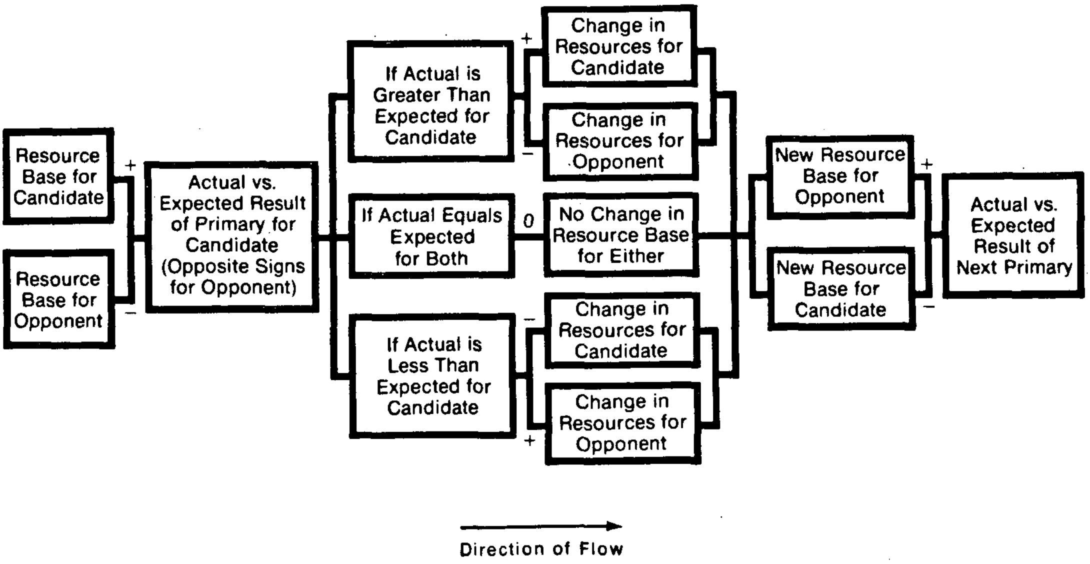
To begin, I assume that there is a set of m candidates competing for the nomination, say C; where $C_i \in C$, $i=1, \ldots, m$. I assume that it is unambiguous who these m candidates are. In practice, it may not always be the case that identifying this set of candidates is unambiguous (was Ellen McCormack a "significant" candidate in 1976, was Larry Pressler or even Phil Crane in 1980?).

Second, I assume that there is a set of n time periods; $t=1, \ldots, n$, that denote the dates of relevant events (state caucuses, for example, or more commonly, state primaries). Further, I assume that the number of days between, say, two primaries is less important than the fact that one primary follows another. Thus, each period between primaries (ordinarily at least one week in 1976 and 1980) is assumed to be equivalent (time so measured is commonly called "event time").

Third, I take it as axiomatic that candidates want to win the nomination. Therefore, the central goal of the candidate prior to the convention is to win delegate support. As a consequence, my notation will focus on numbers of delegates rather than on, say, numbers of votes won in a primary. Let d_t be the number of delegates to be selected on date t, let d_{it} represent the number of delegates C_i won on date t, let D_{it} be the total number of delegates C_i has won through date t, and let D be the total number of delegates to attend the convention (i.e., $D = \sum_t d_t$). Let $E(d_{it})$ denote the number of delegates C_i expects (and is expected) to win on date t. Let CS_{it} denote C_i's "competitive standing" at time t (to be formalized below), $E(CS_{it})$ represent the expectation of CS_{it} prior to the primary or primaries at time t, and $DS_{it} = CS_{it} - E(CS_{it})$ denote the difference between actual and expected outcome. Finally, let P_{it} represent the probability of C_i winning the nomination as assessed immediately after the primary or primaries on date t.

With these relatively straightforward assumptions and with notational matters out of the way, let us turn to the central assumptions of the model. First, about delegates, etc.:

ASSUMPTION 1. *Since the first goal of presidential candidates is to win the party's nomination, they seek to win the support of delegates. In particular, the probability of attaining that goal is assumed to be some function of delegate awards from primaries and caucuses.*


Resource Base for Candidate
+
Resource Base for Opponent
−
Actual vs. Expected Result of Primary for Candidate (Opposite Signs for Opponent)
If Actual is Greater Than Expected for Candidate
+
Change in Resources for Candidate
−
Change in Resources for Opponent
If Actual Equals Expected for Both
0
No Change in Resource Base for Either
If Actual is Less Than Expected for Candidate
−
Change in Resources for Candidate
+
Change in Resources for Opponent
New Resource Base for Opponent
+
New Resource Base for Candidate
−
Actual vs. Expected Result of Next Primary
Direction of Flow

Source: John H. Aldrich (1980), *Before the Convention: Strategies and Choices in Presidential Nomination Campaigns,* Chicago: University of Chicago Press, Fig. 5.2.

Figure 1. A Representation of the Model of Campaign Dynamics

To be more specific, I now define the measure of competitive standing, CS_{it}:

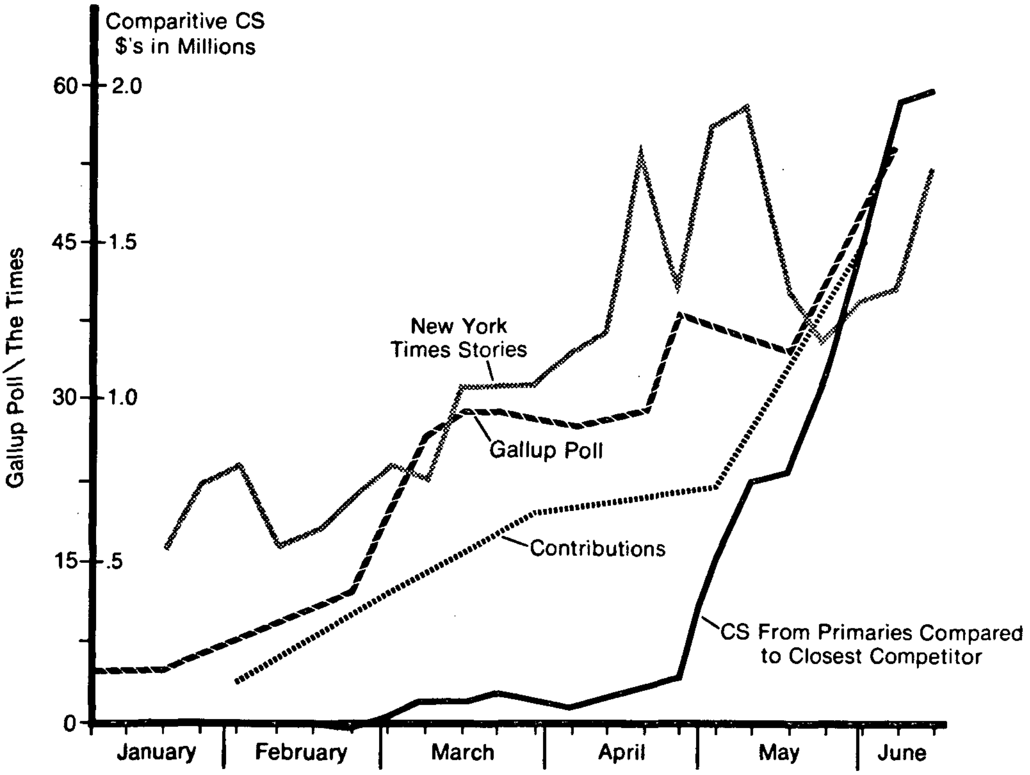
$$CS_{it} = \frac{D_{it} - \sum_{j \neq i} D_{jt}}{D - \sum_{j=1}^{m} D_{jt}} . \qquad (1)$$

In words, CS_{it} equals the number of delegates C_i has won to date less those won by all opponents divided by the "pool" of remaining delegates (i.e., the total number of delegates to be selected less those already committed to some candidate, including C_i). At time t, the denominator, or "pool" of available delegates, includes all delegates as yet unselected, all chosen as uncommitted, and all delegates (if any) chosen as committed to a candidate who has subsequently withdrawn or to some other individual, such as a "favorite son." While I believe the particular properties of CS_{it} are useful (see Aldrich, 1980), I will not use these properties here. That is, the term CS_{it} will appear in equations, but its definition of Equation (1) will not be used.

Second, I assume that, at least after the event(s) at $t-1$, expectations can be formed about delegates expected to be won or lost at time t, $E(d_{it})$:

ASSUMPTION 2. *Candidates can make and agree upon the division of delegates expected to be won by each, at least for the immediately subsequent delegate selection procedures.*

It is rather unproblematic that some sort of expectations can be formed about the next primary. It may seem more problematic to assume that all agree on these expectations. Each candidate forms at least one scenario for winning the nomination, and at most one of these scenarios can be true. Yet these scenarios are for public consumption, and thus need not reflect the candidate's best assessment of the situation. Further, there is likely to be more



Source: John H. Aldrich (1980), *Before the Convention: Strategies and Choices in Presidential Nomination Campaigns,* Chicago: University of Chicago Press, Fig. 5.1.

Figure 2. Jimmy Carter's "Momentum" in 1976

agreement in scenarios about the most proximate primary yet to come. More to the point, the candidates may not have much control over these expectations (indeed, reality may not either). Matthews' perspective quoted above is virtually conventional wisdom. Many believe that the media are central to the setting of expectations. What is important is that $E(d_{it})$ become formed and serve as the benchmark. If delegate expectations can be formed, these can be fed into whatever equation serves as a measure of competitive standing to form an expectation about what CS_i will look like after date t, $E(CS_{it})$. The crucial differential, then, is the difference between expected and actual results:

$$DS_{it} = CS_{it} - E(CS_{it}). \tag{2}$$

When Equation (2) is positive, C_i has exceeded expectations and has done so relative to how opponents fared.

The relationships studied in this article concern dynamics. Since delegates are selected in blocs (namely, at the time of each primary or caucus procedure), and since most are selected as committed to some candidate, difference equations seem a more appropriate dynamic formalism than differential equations.[6] Thus the dynamic elements of a campaign such as those called "momentum" are taken to mean changes in the probability that C_i will win the nomination. If $\Delta P_{it} = P_{it+1} - P_{it}$, the first difference of C_i's nomination probability, is positive, his or her chances are increasing. More formally, "momentum," "decay," and "stability" may be defined in the following ways:

1. C_i is said to have "momentum" if $\Delta P_{it} > \eta$; $\eta > 0$.
2. C_i's campaign is said to be in "decay" if $\Delta P_{it} < -\eta$. (3)
3. C_i's campaign is "stable" if $-\eta \leqslant \Delta P_{it} \leqslant \eta$.

The term η is added to give substance to the term "significant," as in "momentum represents a significant increase in P_{it}."[7]

Changes in P_{it} are traced to a candidate exceeding or failing to meet expectations:

ASSUMPTION 3. (a) *A candidate's probability of winning the nomination increases (decreases/stays constant) if he or she exceeds (fails to meet/exactly meets) expectations,* ceteris paribus. (b) P_{it} *also is affected by how* C_i*'s opponents fare. Thus, if an opponent exceeds expectations,* C_i*'s nomination probability decreases,* ceteris paribus.

Assumption 3 can be stated more formally. If ϕ_1 denotes some (arbitrary) monotonic increasing function, then

$$\Delta P_{it} = \phi_1\ (DS_{it}, - DS_{jt}) \text{ for all } j \neq i. \tag{4}$$

Obviously, if DS_{it} is large enough or if the DS_{jt}'s are sufficiently negative, ΔP_{it} could exceed η, and C_i's campaign could have momentum.

The next step in the model involves resources. Let R_{it} represent the bundle of time variable resources that C_i can obtain and bring to bear on the primary at date t. Recall that some resources, broadly conceived, enter the model as a constant factor, some as a stochastic factor, and some as being taken into account in establishing expectations. Others, such as money, are measured by R_{it}.

The central assumptions about resources in the sense of R_{it} are:

ASSUMPTION 4. DS_{it}, *the extent to which a candidate exceeds (or fails to meet) expectations in a monotonic increasing function of the resources* C_i *invests in the primary or primaries at time* t, *and a monotonic decreasing function of the resources spent by opponents in that primary or those primaries (plus a stochastic term). In short,* ceteris paribus, C_i *will do better with more resources than with fewer, and will do better if the opponents bring to bear fewer rather than more resources.*

If ϕ_2 again denotes some monotonic increasing function:

$$DS_{it} = \phi_2\ (R_{it}, - R_{jt}) \text{ for all } j \neq i. \tag{5}$$

Second:

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[6] Barbara Salert and Carol Kofeld (1979) argue that the choice between difference and differential equation forms should be based on more than just the apparently "natural" form. In particular, the different structures can lead to quite different patterns of behavior. In this case, the major results are identical in both formulations.

[7] η might be a constant. More realistically, η should be contingent on the value of P_{it}, say, $\eta = \frac{\Delta P_{it}}{P_{it}}$. In this case what is "significant" depends upon how close P_{it} is to one. The closer the candidate is to victory, the smaller the increase necessary to constitute a "significant" increase.

ASSUMPTION 5. *If a candidate exceeds expectation at time* t, ceteris paribus, *he or she will be able to generate resources at an increasing rate. If* C_i *fails to meet expectations, he or she will be able to generate resources at a decreasing rate. If he or she just meets expectations,* C_i *will be able to generate resources at a constant rate. The impact of the other candidate(s) work(s) in exactly the opposite direction in terms of* C_i*'s rate of resource acquisition,* ceteris paribus.

If ϕ_c denotes another monotonic increasing function:

$$\Delta R_{it} = \phi_3 (DS_{it}, - DS_{jt}) \text{ for all } j \neq i. \qquad (6)$$

If DS_{it} is positive, *ceteris paribus,* C_i will have more resources at time t+1 than at time t, so that ΔR_{it} will be positive.

The general Equations, (4), (5), and (6), state the dynamic form of the terms in this system of equations. In the next sections, these general equations are specified algebraically. The next section examines the two-candidate nomination campaign.

The Two-Candidate Model

Let i and j denote the two candidates. The specific mathematical assumptions are that the generic functional forms of Equations (5) and (6) can be specified as a pair of simple, first-order, linear difference equations with (time) constant parameters. Therefore, the two-candidate version of Equation (5) may be written as:

$$\begin{aligned} DS_{it} &= a_{11} R_{it} - a_{12} R_{jt} + c_i + e_{it}; \\ DS_{jt} &= -a_{21} R_{it} + a_{22} R_{jt} + c_j + e_{jt}. \end{aligned} \qquad (7)$$

The specification of Equation (6) for this special case follows analogously:

$$\begin{aligned} \Delta R_{it} &= b_{11} DS_{it} - b_{12} DS_{jt} + d_i + u_{it}; \\ \Delta R_{jt} &= -b_{21} DS_{it} + b_{22} DS_{jt} + d_j + u_{jt}. \end{aligned} \qquad (8)$$

The role of the stochastic terms will be studied later, and thus will be dropped for now. The terms such as c_i and d_i are constants, summarizing, *inter alia,* the time-constant resources described above. As usual in these types of equations, the constant terms play no role in the stability of these equations or lack thereof. They do, however, specify what the actual equilibrium(a) value(s) of the system will be.

Two lines of inquiry will be pursued since P_{it} and ΔP_{it} are unobservable in any precise sense. I want to investigate the nature of the movement of delegates, via Equation (7), and of resources, via Equation (8). Note that these two pairs of equations can be considered as one block of (four) simultaneous equations, the results of which, then "feed into" the right-hand side of Equation (4).

Resource Dynamics in the Two-Candidate Case

Equation (7) can be substituted into Equation (8) to state ΔR_{it} in terms of prior resources:

$$\begin{aligned} \Delta R_{it} &= b_{11}(a_{11} R_{it} - a_{12} R_{jt} + c_i) \\ &\quad - b_{12}(-a_{21} R_{it} + a_{22} R_{jt} + c_j) + d_i; \\ &= (b_{11} a_{11} + b_{12} a_{12}) R_{it} \\ &\quad - (b_{11} a_{21} + b_{12} a_{22}) R_{jt} \\ &\quad + (d_i + b_{11} c_i - b_{12} c_j); \\ \Delta R_{jt} &= -(b_{21} a_{11} + b_{22} a_{21}) R_{it} \\ &\quad + (b_{21} a_{12} + b_{22} a_{22}) R_{jt} \\ &\quad + (d_j + b_{22} c_j - b_{21} c_i). \end{aligned} \qquad (9)$$

Equation (9) specifies changes in resources from time t to time t+1 as a function of the resources invested in the primary at time t by the two candidates. The combinations of parameters combine the effects of such factors as the role that the resources C_j spends in the primary on C_i's delegate returns $(-a_{12})$ and as these two, in turn, influence the rate of C_i's resource acquisition—the $+b_{12}a_{12}$ term in Equation (9). Nonetheless, Equation (9) remains a pair of simultaneous, linear difference equations that are non-homogenous and, by assumption, constant in the parameters. These characteristics can be seen by letting $\alpha_{11} = (b_{11}a_{11} + b_{12}a_{21})$ and so on, yielding the rather simple form:

$$\begin{aligned} \Delta R_{it} &= \alpha_{11} R_{it} - \alpha_{12} R_{jt} + \alpha_i; \\ \Delta R_{jt} &= -\alpha_{21} R_{it} + (1 + \alpha_{22}) R_{jt} + \alpha_j. \end{aligned} \qquad (10)$$

The constants α_{11} and α_{22} are essentially "feedback" terms, while $-\alpha_{12}$ and $-\alpha_{21}$ denote the magnitude of "reaction" of C_i's resource base to the opponent's actions. Equation (10) can also be cast into a form that looks at the amount of resources C_i and C_j gather at time t+1 (since $\Delta R_{it} = R_{it+1} - R_{it}$, the latter term being moved to the right-hand side):

$$R_{it+1} = (1 + \alpha_{11})R_{it} - \alpha_{12} R_{jt} + \alpha_i; \qquad (11)$$
$$R_{jt+1} = -\alpha_{21} R_{it} + (1 + \alpha_{22}) R_{jt} + \alpha_j.$$

The solution to this set of equations is developed in the appendix (see also Goldberg, 1958). Basically, there are two solutions to Equations (10) or (11) (and, in general, m solutions to m first-order linear difference equations). What is of interest is the so-called "general solution," from which the general properties of the equations can be investigated (this differs from the "specific solution" which yields numerical results given particular values of the parameters and the variables). If the two solutions are denoted $\lambda^{(q)}$ (where $q = 1, 2$), it is shown in the appendix that the two values, $\lambda^{(q)}$ are:

$$\lambda^{(q)} = \tfrac{1}{2}(2 + \alpha_{11} + \alpha_{22} \pm \sqrt{\alpha_{11}^2 + \alpha_{22}^2 - 2\alpha_{11}\alpha_{22} + 4\alpha_{12}\alpha_{21}}). \qquad (12)$$

Given values of $\lambda^{(1)}$ and $\lambda^{(2)}$, the paths of changes in resources flows over time can be assessed. The major mathematical questions about the behavior of R_{it} and R_{jt} over time are as follows: (a) are they stable or unstable (is there "explosive" change or is there convergence to some equilibrium value of resources), and (b) is the time path monotonically increasing/decreasing or does it oscillate? These questions can be answered definitively from Equation (12). In particular, the dynamics are given by the size and sign of the $\lambda^{(q)}$'s. A time path will be stable if the largest value of $\lambda^{(q)}$ in absolute value is less than one (cf. Goldberg, 1958):

$$\max |\lambda^{(q)}| < 1. \qquad (13)$$

If the inequality is reversed, there is an "explosive" or unstable time path. Further, if both $\lambda^{(q)}$'s are positive, there will be no oscillations, and if one is positive and the other negative, the larger one will capture the dominant characteristics of the time path.

In the appendix it is shown that in this case, the roots of the characteristic equation will be real, and the larger (and hence dominant) root will be positive. Therefore, there will be no oscillatory behavior in general, and if any exists (when the term in the radical of Equation (12) is larger than $2 + \alpha_{11} + \alpha_{22}$), it will damp out fairly quickly due to the dominance of the positive root.

With this background, the following two propositions of substantive interest can be proven (see appendix):

THEOREM 1. *If the two-candidate campaign is described by Equations (4), (7), and (8), the resource equation given by Equation (9)* cannot *be stable, or the model is degenerate (i.e., all coefficients are zero). That is, there is no stable equilibrium value of the rate at which resources are generated. Change will occur, and it will occur at an "explosive" rate, since max* $\lambda^{(q)} > 1$.

THEOREM 2. *Theorem 1 applies to both candidates in* exactly *the same form.*

The substantive thrust of these two propositions may be summarized briefly. Each candidate is faced with an unstable situation. Either the candidate's resources increase without bound, or they decrease without bound. The two candidates are in parallel situations, such that the magnitudes of such forces are precisely the same for each candidate.

Even though there is no stable equilibrium value of ΔR_{it}, there remains the possibility that resource acquisition will appear stable (without recourse to fortuitous values of the stochastic term). This appearance of stability will occur when the "feedback" and "reaction" coefficients are equal:

$$\alpha_{11} R_{it} = \alpha_{12} R_{jt} \text{ and } \alpha_{21} R_{it} = \alpha_{22} R_{jt}. \qquad (14)$$

If so, Equation (10) reduces to its equilibrium value:

$$\Delta R_{it} = \alpha_i \text{ and } \Delta R_{jt} = \alpha_j. \qquad (15)$$

That is, the change in resources for each candidate will be constant over time. This circumstance might occur in a balanced, competitive, two-candidate race, such as the 1976 Republican campaign. In that case, Ford and Reagan began the campaign with large and roughly equivalent resource bases, and neither was able to gain a significant electoral edge in the primaries. If so, resource acquisition should have been relatively stable over time, and, more to the point, the time paths in resource flows to the two candidates should have been essentially identical. Available evidence suggests that this is a reasonable description of that campaign (see Aldrich, 1980, pp. 119–24). However, it must be emphasized that this stability is one of *appearances* only. Should there be any significant deviation from this knife-edge balance, the unstable dynamic forces would take over.

The duration of U.S. primary campaigns is finite and small ($t = 14$ in 1976). Thus, it is important to ask *how* unstable the model is. The answer depends upon the magnitude of the coefficients involved, and I have avoided exploiting properties based on specific values in the absence of reasonable estimates. It should be noted, however, that the degree of instability is measured by the extent to which the right-hand side of Equation (12) exceeds one for the larger of $\lambda^{(1)}$ and $\lambda^{(2)}$.

Delegate Dynamics in the Two-Candidate Case

The second line of inquiry, that of delegate dynamics, can be followed more quickly given the similarity in mathematical structure to resource dynamics. While delegate dynamics involve unobservables, at least at present, the dynamics are more directly relevant to the time path of the probability of attaining the nomination and hence to "momentum" and related concepts. Equation (7) can be transformed to first difference equations by subtracting it from the formulas for DS_{it+1} and DS_{jt+1}, obtaining the pair of homogeneous equations:

$$\begin{aligned} \Delta DS_{it} &= a_{11}\Delta R_{it} - a_{12}\Delta R_{it}; \\ \Delta DS_{jt} &= -a_{21}\Delta R_{it} + a_{22}\Delta R_{jt}. \end{aligned} \tag{16}$$

This formulation is consistent with what commentators call the "vicious cycle" of low resources minimizing electoral success, and low electoral success in turn making the acquisition of resources difficult (see, for example, White, 1969, pp. 438–43).

Equation (8) can be substituted into the right-hand side of Equation (16) to obtain, with suitable rearrangement:

$$\begin{aligned} \Delta DS_{it} = {} & (a_{11}b_{11} + a_{12}b_{21})\,DS_{it} \\ & - (a_{11}b_{12} + a_{12}b_{22})\,DS_{jt} \\ & + (a_{11}d_i - a_{12}d_j); \\ \Delta DS_{jt} = {} & -(a_{21}b_{11} + a_{22}b_{21})\,DS_{it} \\ & + (a_{22}b_{12} + a_{22}b_{22})\,DS_{jt} \\ & + (a_{22}d_j - a_{21}d_i). \end{aligned} \tag{17}$$

The parallel with the resource equations is strong; using β's to denote the coefficient combinations, the exact analogue of resource Equation (14) is:

$$\begin{aligned} \Delta DS_{it} &= \beta_{11}DS_{it} - \beta_{12}DS_{jt} + \beta_i; \\ \Delta DS_{jt} &= -\beta_{21}DS_{it} + \beta_{22}DS_{jt} + \beta_j. \end{aligned} \tag{18}$$

From this point, the analysis is the same as that done for resource dynamics, leading to the following two theorems (see appendix for proof):

THEOREM 3. *If the model leading to Equation (18) is correct, the "success-generating function," given by Equation (18)* cannot *be stable. That is, deviations from expectations must either increase or decrease without bound.*

THEOREM 4. *Theorem 3 applies to both candidates in exactly the same form.*

The "appearance of stability" argument applies more obviously to this context. If the two candidates exactly meet expectations, then the system will be at an unstable balance point. For example, Ford and Reagan basically tied in the New Hampshire primary in 1976, a situation that was just about in line with predictions. While Ford's tiny edge did have some effect (see Aldrich, 1980), essentially expectations were realized. However, once disturbed from this balance point, the third and fourth theorems would come into play. Finally, the tie to P_{it} is as follows:

PROPOSITION 1. *From Equation (4), once a balanced point is not met, Theorems 3 and 4 imply that one candidate's chances for the nomination will increase, eventually reaching a point where that candidate's campaign will have "momentum." At some point, the opponent's chances will "decay."*

For example, the first few Republican primaries in 1976 were interpreted as small (but consistent) victories for the incumbent. By North Carolina, Reagan was at a decided disadvantage, with Republican leaders asking him to withdraw, for example. That is, what appeared to be a "competitive" (i.e., balanced) campaign in light of the very small Ford victory in New Hampshire had become more asymmetric. Further, Reagan's money was essentially gone, and he no longer had the resources to compete as he desired in the Wisconsin primary which followed North Carolina's. The night of his victory in North Carolina (which was perceived as an upset), Reagan and his staff searched for a change in his campaign strategy. He withdrew from Wisconsin, appeared on national television, changed the policy thrust of his campaign to emphasize foreign and defense issues, and appealed for money. He did not compete in any primaries for more than a month, but focused his efforts on the Texas primary of May 1. These alterations in strategy seem quite consistent with a search for exogenous "shocks" in hopes of altering the dynamics of the campaign. Finally, his financial status by the time of the Wisconsin campaign period is wholly consistent with the model.

Extension to the *m*-Candidate Case and Resource Dynamics

A two-candidate contest for a major party nomination is rare. It is more common to have campaigns with several candidates, such as the Democratic nomination campaigns of the 1970s and the 1980 Republican campaign. The general model was defined for two or more candidates. Only Equations (7) and (8) need to be extended, and they can be so in the obvious way. For the extension of Equation (7), assume simply that the resources of each additional opponent have a negative effect on C_i's chances, while for Equation (8), the parallel assumption is made. Thus, there are m equations in each case, one for each candidate, and the right-hand side of each contains the m variables, one for each candidate. These are as follows for C_i:

$$\begin{aligned} DS_{it} = {} & -a_{i1}R_{1t} - \ldots + a_{ii}R_{it} - \ldots \\ & - a_{im}R_{mt} + c_i + e_{it}, \end{aligned} \tag{19}$$

and

$$\begin{aligned} \Delta R_{it} = {} & -b_{i1}DS_{1t} - \ldots + b_{ii}DS_{it} - \ldots \\ & - b_{im}DS_{mt} + d_i + u_{it}. \end{aligned} \tag{20}$$

These equations can be cast into matrix form:

$$DS_t = A(R_t) + C + E_t, \text{ where}$$

$$DS_t = \begin{bmatrix} DS_{1t} \\ \cdot \\ \cdot \\ \cdot \\ DS_{mt} \end{bmatrix}, A = \begin{bmatrix} a_{11} \ldots -a_{1m} \\ \cdot \qquad \cdot \\ \cdot \qquad \cdot \\ \cdot \qquad \cdot \\ -a_{m1} \ldots +a_{mm} \end{bmatrix}, \tag{21}$$

$$R_t = \begin{bmatrix} R_{1t} \\ \cdot \\ \cdot \\ \cdot \\ R_{mt} \end{bmatrix}, C = \begin{bmatrix} c_1 \\ \cdot \\ \cdot \\ \cdot \\ c_m \end{bmatrix}, \text{and } E_t = \begin{bmatrix} e_{1t} \\ \cdot \\ \cdot \\ \cdot \\ e_{mt} \end{bmatrix};$$

$$\Delta R_t = B(DS_t) + D + U_t, \tag{22}$$

where the matrices are analogous to those as defined above. Ignoring the stochastic terms and substituting Equation (22) into Equation (21):

$$\Delta R_t = B[A(R_t) + C] = BA(R_t) + BC$$

or (23)

$$\Delta R_t = F(R_t) + G, \text{ where } F = BA,\ G = BC.$$

Alternatively, R_{t+1} can be solved for:

$$R_{t+1} = H(R_t) + G, \text{ where}$$

$$H = \begin{bmatrix} (1+f_{11}) \ldots & -f_{1m} \\ \cdot & \cdot \\ \cdot & \cdot \\ \cdot & \cdot \\ -f_{m1} \ldots & (1+f_{mm}) \end{bmatrix}. \tag{24}$$

In general, R_t can be expressed as:

$$\begin{aligned} R_t = {} & H^t(R_o) + (H^{t-1} + H^{t-2} + \ldots \\ & + H + I)G. \end{aligned} \tag{25}$$

What, then, must be true about H to have stability? The answer is much like that for the two-candidate case. The stability properties are completely determined by the characteristic roots of H (i.e., the $\lambda^{(q)}$'s of the two-candidate case). In particular, the characteristic roots, $\lambda^{(q)}$ where $q = 1, 2, \ldots, m$, must satisfy:

$$\max_q |\lambda^{(q)}| < 1. \tag{26}$$

This condition, of course, is the obvious generalization of that for the two-candidate case. It requires that no characteristic root exceed one in absolute value. The comments made in the earlier section about sign and size of roots and their effects on the dynamic properties generalize here as well. That is, the two-candidate case is indeed a special case of the *m*-candidate model.

It is proven in the appendix that the *m*-candidate contest will be unstable (max $|\lambda^{(q)}| > 1$), and it is argued there that the degree of instability increases as m increases (i.e., max $|\lambda_q|$ increases as m increases):

THEOREM 5. *If Equations (4), (5), (6), (21) and (22) describe the* m*-candidate contest, then the model of resource dynamics is necessarily unstable (max* $|\lambda^{(q)}| > 1$*).*

PROPOSITION 2. *As* m *increases, the degree of instability increases (*$\max_q |\lambda^{(q)}|$ *exceeds one by greater amounts as* m *increases).*

Consequently, the larger the field of contenders, the more unstable the situation. That is, in large fields attention tends to narrow quickly to one or a very few candidates, and they are able to garner the lion's share of time-variable resources. The rest find it increasingly harder to gather sufficient resources to conduct viable campaigns and, presumably, they are eventually forced out of the competition.

Delegate Dynamics for the *m*-Candidate Case

The equations describing the dynamics of delegate selection procedures generalizes straightforwardly from the two to the general *m*-candidate case, and builds from the parallels with the resource equations. The basic *m*-candidate equations are repeated here:

$$DS_t = A(R_t) + C + E_t \tag{21}$$

and

$$\Delta R_t = B(DS_t) + D + U_t. \tag{22}$$

Again it is possible to construct the first difference of Equation (21), which is (ignoring E_t and U_t):

$$\Delta DS_t = A(\Delta R_t). \tag{27}$$

Next, solve for ΔDS_t in terms of DS_t by substitution of Equation (22) into Equation (27):

$$\begin{aligned} \Delta DS_t &= A[B(DS_t) + D]; \\ \Delta DS_t &= AB(DS_t) + AD. \end{aligned} \tag{28}$$

If $J = AB$ and $K = AD$, then:

$$\Delta DS_t = J(DS_t) + K. \tag{29}$$

Equation (29) can be expressed in terms of DS_{t+1}:

$DS_{t+1} = M(DS_t) + K$, where

$$M = \begin{bmatrix} (1+j_{11}) \dots & -j_{1m} \\ \cdot & \cdot \\ \cdot & \cdot \\ \cdot & \cdot \\ -j_{m1} \dots & (1+j_{mm}) \end{bmatrix}. \tag{30}$$

Clearly, the same stability conditions hold for this equation as for the resource equations. Therefore, the same propositions apply in this section as the last, especially (see appendix for proof):

THEOREM 6. *If Equations (4), (5), (6), (21) and (22) describe the* m*-candidate model, then the* m*-candidate, expected delegate dynamics are* unstable.

PROPOSITION 3. Ceteris paribus, *the degree of instability will be positively related to the number of candidates.*

Finally, from Equation (4), it follows that:

PROPOSITION 4. *Given Theorems 1–6 and Propositions 1–3, in general there are no* stable *equilibria for* P_{it}. *Thus we should expect one or more candidates' campaigns to be describable as "having momentum" and one or more candidates' campaigns to be describable as "being in decay."*

Some Additional Theoretical Considerations

There is, essentially, one major theorem proven in this article, and that is that the linear, first-difference formulation of Equations (4)–(6) is necessarily unstable. What are called Theorems 5 and 6 are actually special cases of the theorem proven in the appendix. I believe it was worthwhile, however, to structure the analysis as presented, in particular by analyzing resource dynamics and delegate (or vote) dynamics separately. Resource flows, who gets how much and when, are at least in principle measurable, as in Figure 2, for example. These equations, that is, are in principle estimable (albeit not at all straightforwardly). Moreover, when candidates withdraw from active competition, the proximate rationale is based ordinarily on resources, per se. In short, resource generation is substantively interesting and, even with current technology, is admissible to scientific inquiry. Delegate and, particularly, expectational dynamics are much less obviously measurable. They are, however, one of the dominant topics of journalistic and other accountings of events, both as they happen and in retrospect (cf. Witcover, 1977). It must be remembered, however, that the two aspects of campaign dynamics, while separable for study, are integrally related to the whole of the model. As proven in Theorems 2 and 4 (and as can be proven easily for the *m*-candidate case), if resources possess an unstable dynamic, then delegate/expectational components of the model do, too, and vice versa. Similarly, if it were true that one aspect were stable, the other would be as well. That is, the entire system of equations is either stable (which it is not) or unstable.

As a second point, Theorems 1 and 3 are, literally, special cases of Theorems 5 and 6. However, some slightly different properties are possible in the event there are three or more candidates. Let me suggest two such comparisons.

1. R_{it} in general can be positive for more than one candidate only if $m > 2$. In turn, this means that it is logically possible for more than one candidate to "have momentum" if $m > 2$.[8]
2. While the equation system is unstable, it is possible for one (or more) candidate to have a stable R_{it} over a finite interval of time. Suppose there are three candidates, C_i, C_j and C_k, and suppose for simplicity that all coefficients are equal. Suppose further that $R_{it} = -R_{jt}$. Then, on substitution into the equation for R_{kt}, these two terms have the effect of "cancelling" each other. Then, what, say, C_i "gains" comes at the "expense" of C_j, leaving C_k unaffected.[9]

As a final theoretical consideration, consider a special instance of a two-candidate campaign. Suppose that $(\alpha_{11}\ \alpha_{22})$ is approximately equal to $(\alpha_{12}\ \alpha_{21})$ (and this holds for their β counterparts). This instance might describe two very different campaigns. First, it might describe a competitive campaign, if by "competitive" we mean that the actions taken by the two candidates are equally material in affecting outcomes, perhaps such as the 1976 Republican campaign. Second, such an instance might arise in almost exactly the opposite circumstance, where one candidate is dominant. If so, the two products might be equal since, say, α_{11} and α_{12} are both large while α_{22} and α_{21} are both small. For instance, in 1972 the fortunes of a John Ashbrook, say (ignoring Pete McClosky), might be assumed to be based more on what Richard Nixon did than on his own actions and results.

In either case, if the two "feedback" and "reaction" coefficient products are assumed equal, Equation (12) is solvable, yielding the two values of $\lambda^{(q)}$ of:

$$\lambda^{(1)} = 1; \lambda^{(2)} = 1 + \alpha_{11} + \alpha_{22}.$$

The extent of instability, then, is equal to the size of the sum of the "feedback" terms, and any "dampening" effect of the opponent is irrelevant.

The above suggests a possible interpretation of the 1968 and 1972 Democratic primaries in New Hampshire. In both cases, the presumed "front-runner" (dominant candidate?) defeated a challenger thought to have an outside chance at the nomination at best. Yet what was important was that neither Johnson nor Muskie lived up to *expectations.* The immediate and "direct" effect was to terminate those two candidacies effectively and rather quickly. While McCarthy and McGovern were ultimately the beneficiaries (for a time), McGovern, at least, developed little more popular support until *after* he had a clear first-place finish in the Wisconsin primary (consistent with an assumed small direct "feedback," see Aldrich, 1980, pp. 133–35). In both cases, the general flow of the nomination could hardly be called "stable."

Discussion

One general point worthy of note is that this model is in part consistent and in part in conflict with other equilibrium-based models of nomination campaigns, particularly that proposed recently by Brams (1978; basically, of course, these are simply different models and thus difficult to compare). Brams employed a Downsian (unidimensional) spatial model in which he found (a) a "winnowing-out" process in multi-candidate campaigns, much like that found here, but (b) points of stable equilibria in two-candidate contests. The divergence along the line of point *b* rests, it seems to me, on the difference between time-constant "resource" (as in citizen policy preferences for Brams) and time-variable "resources" as used here. While I believe these two models are not incompatible, the test ultimately will be based on whether or not it is necessary for candidates to acquire sufficient amounts of such time-variable resources as money to make other resources such as issue positions effective for obtaining electoral success.

The propositions developed in this article suggest a number of important consequences.

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[8] In Aldrich (1980, pp. 124–25), it is suggested that both Carter and Brown "had momentum" simultaneously in 1976, at least in terms of resource acquisition.

[9] Udall might have been in that position in 1976. An algebraic illustration of this point is in the appendix.

Perhaps the most important is "decisiveness." The pre-convention campaign period as it is currently constructed should be expected to be "decisive" in the sense of effectively determining the nominee prior to the opening of the convention itself. This point is particularly striking when the nomination begins with a field of 8, 10 or even more candidates, such as in the 1972 and 1976 Democratic campaigns and the 1980 Republican campaign. Contrary to conventional wisdom, the prospects of a "deadlocked convention" need not be great. Obviously, the propositions produced here do not guarantee the emergence of precisely *one* candidate before the convention, but they do imply a rapid narrowing of the field. If the field is narrowed to, say, exactly two candidates quickly enough, then, according to the first few theorems, the pre-convention campaign will be decisive. Since the intention of reforms, such as those leading to the proliferation of primaries, was to allow the public to play a more important role in choosing presidential nominees, that intention was realized. At the very least, party leaders have and can expect relatively few instances in which they can "control" presidential nominations.

If the pre-convention campaign tends to be decisive, the strategy followed by, say, Humphrey in 1976 and apparently to be followed by Ford in 1980 as an inactive but available candidate to turn to should all active candidates fail, is a less viable strategy now than in preceding decades. In particular, it is less likely that all active candidates will fail.

Another consequence of this model concerns the financial reforms of the 1970s. These reforms were enacted to regulate the influence of money in elections. Obviously, limitations placed on individual contributions have minimized the influence of one or a very few "fat cats" on presidential candidates. Similarly, the full federal funding of general election campaigns has been effective. However, presidential nomination campaigns are only partially funded by federal sources through a "matching funds" provision. Paradoxically, to the extent that contributions are matched and to the extent that the ability to raise money is dynamically variable, the matching provisions *exaggerate* the impact of differential resource raising and give substance to the claim that "to the victor belong the spoils." The irony, then, is that matching funds act as a multiplier effect, *increasing* the role of money in presidential nomination campaigns. *Full* federal funding of these campaigns, of course, would eliminate money as one of the time-variable resources as modeled here (raising instead complicated questions of qualification for funding).[10]

Three features of the model deserve additional discussion. First, the stochastic term that was built in but ignored can play a critical role in the model. It was included because it seems to be a part of the nature of the process. Its inclusion leads to a major methodological problem, however. In particular, reasonable estimation of the parameters of the model will be extremely difficult. Even starting with Gauss-Markov type assumptions, the stochastic terms end up being complicated functions of the model's parameters, and, thus, the estimation of these models will require great care.

Theoretically, the nature of the stochastic term can be crucial, changing the particular values of DS_{it} and R_{it}, and hence changing the dynamics. This point is especially true early in the campaign, when all CS_{it} consist of very few actually attained delegates. As this proportion increases, the dynamic is more and more a function of resolved delegate selections, and thus more and more immune to stochastic elements. This feature of the model corresponds to observations of real campaigns. There is a great degree of uncertainty early in the campaign with respect both to the outcome of specific primaries and, more importantly, to the outcome of the nomination. As time goes by, the expected outcomes of specific primaries often remain in doubt (but even here the uncertainty is usually less) but the general thrust of the campaign is more obvious.

A second way of interpreting the stochastic term is as an "exogenous shock," an event that is external to the model (i.e., unpredictable within the model), but that may be very large in magnitude on occasion. For example, the Federal Election Commission was unable to disburse federal funds, in the wake of the *Buckley* v. *Valeo* decision rendered by the U.S. Supreme Court, from March 22 to May 17, 1976, covering a large and crucial period of the campaign. While all participants were affected, of course, the effects were not necessarily

[10] The ceilings on expenditures in each state and for the nation as a whole that are mandated for those accepting matching funds can, in principle, modify the dynamics. As candidates capture a positive resource dynamic, they might find that they can gather more money than they are allowed to spend. Thus they might be unable to capitalize fully on the dynamics. Similarly, spending too much early in the campaign might mean that these candidates will be unable to compete as effectively in later primaries, should significant opposition remain. In short, a budget constraint might dampen a candidate's momentum (a near "what if" for Reagan in 1980).

equal. Two Democrats, Harris and Jackson, attributed the timing (at least) of their withdrawal from active candidacy to the inability to receive federal funds to which they were entitled in a timely fashion.

Another and more important class of "exogenous shocks" is the deliberate strategy of candidates. If, for example, they were fully aware of this model and accepted it as true, candidates in a disadvantageous position might be expected to search for strategies that would modify or change the dynamics. The already mentioned actions of Reagan following his narrow but surprising victory in North Carolina would apply here. That is, his unprecedented national television address and related decisions may be interpreted as an attempt to capitalize on what was no longer quite such a dismal prospect. While the situation was not hopeless, unusual actions were called for to complete the reversal in his fortunes.

The second feature to be discussed is the expected delegate term. The general conception that underlies the model is that candidates can form some "reasonable" expectations about where and how many delegates can be won prior to the campaign itself. Two interpretations of this set of expectations can be made. First, it could be a "realistic" expectation, based on polling data, etc. An alternative interpretation is that it is the candidate's best guess about what is needed to win the nomination and how that majority can be fashioned. That seems to be the general thrust of what candidates try to do, and how that is interpreted by the media. Thus, for example, for Jackson to have won in 1976, he was expected to have to do "well" in industrial states like New York and Pennsylvania and build his coalition around that nucleus. Carter had to defeat Wallace to solidify his hold on the South, and so on. If this form of creating a "most likely" winning coalition is the source of expectations, then an important asymmetry is built into the multi-candidate contest. In a two-candidate campaign, both candidates can formulate expectations about where 50 percent of the delegates could be won without coming into conflict. In principle, both can meet expectations and have a "stable" competitive contest. With three candidates, it must be the case that no more than two candidates can meet expectations. The larger the number of candidates, the greater the number who cannot possibly meet "minimal winning" expectations, and consequently the "winnowing-out" forces should be even stronger.

One final point of discussion concerns the range of applicability of the model. The (implicit) assumption has been that all primaries (and possibly caucuses) are affected by and, in turn, affect the dynamics of the campaign. However, it is possible to assume that a candidate's fortunes are affected materially only by events in which he or she competes against at least one major opponent. For example, Reagan did not intend to contest and in fact did not compete in the New York, Pennsylvania and Massachusetts primaries, among others. Ford obtained the support of nearly all the delegates selected in these primaries, and these delegate totals obviously helped move him towards his ultimate victory. However, the fact that Reagan did not challenge Ford in these states meant that Ford's "victories" and Reagan's "defeats" did not affect the dynamics of the campaign for either of them. Essentially, by failing to compete, Reagan's "expectations" were zero. Similarly, Reagan's withdrawal from Wisconsin, even at the last minute, meant that coming in second (and winning no delegates) was not interpreted as a "defeat."

If the model is assumed to apply to a candidate (a) only where he or she competed actively and (b) only where he or she competed against at least one other active opponent, then the model is changed from its present form. In particular, the decision of where to compete becomes a variable that can best be modeled through, say, game theory. The strategy game, then, becomes one of maximizing the probability of nomination, contingent on the variables and their interactions as modeled here (cf. Aldrich, 1980, pp. 137–54, for an example). The point, of course, is that the dynamics are a "fact" of the process that candidates can use in formulating their campaign strategies and behavior. This apparently mechanistic dynamic process simply provides the basis for studying the politics of nomination campaigns.

Appendix.
Proof of Theorems 1, 3, 5, and 6

Here it is proven that the resource equations are necessarily unstable for the general, m-candidate model (where $m \geqslant 2$). Thus, Theorems 1 and 5 are proven. It follows by analogous reasoning that Theorems 3 and 6, concerning DS_{it}, hold as well. The proof consists of showing that a portion of the necessary conditions for max $|\lambda| < 1$ cannot hold.

The characteristic equation for λ is the following m^{th} degree polynomial:

$$p_0\lambda^m + p_1\lambda^{m-1} + \ldots + p_{m-1}\lambda + p_m = 0.$$

Samuelson reports (1966, p. 690) the Routhian necessary and sufficient conditions for stability, as written "in a slightly more convenient" fashion by Hurwitz and by Frazer and Duncan. The first two of the necessary (as well as sufficient conditions are:

$$p_0 > 0 \text{ and } p_1 > 0.$$

By convention, p_0 is set positive. I will show, therefore, that the two coefficients must be of opposite sign, regardless of the size of m.

The characteristic equation also may be written as:

$$\begin{vmatrix} (1+\alpha_{11}-\lambda) & -\alpha_{12} & \dots & -\alpha_{1m} \\ -\alpha_{21} & (1+\alpha_{22}-\lambda) & \dots & -\alpha_{2m} \\ \vdots & & & \vdots \\ -\alpha_{m1} & -\alpha_{m2} & \dots & (1+\alpha_{mm}-\lambda) \end{vmatrix} = 0.$$

The evaluation of this determinant yields the coefficients $p_0, \dots, p_m$.

The first step is to show that p_0 and p_1 are based on the product of the diagonal elements of the matrix whose determinant is written above, i.e., on:

$$\prod_{i=1}^{m} (1+\alpha_{ii}-\lambda).$$

Let the entry in the matrix be written a_{ij}, where if $i=j$, $a_{ii} = (1+\alpha_{ii}-\lambda)$; while, if $i \neq j$, $a_{ij} = -\alpha_{ij}$; and let the matrix be denoted as A.

A standard method of computing a determinant is to expand by cofactors. The determinant, then, becomes:

$$\det A = \sum_{j=1}^{m} a_{ij}A_{ij},$$

where A_{ij} is the cofactor of a_{ij} (i.e., the determinant of A after deleting row i and column j). Thus, the determinant of A is the product of $a_{i1}A_{i1}$, plus, . . . , plus $a_{im}A_{im}$. Without loss of generality, let $i = 1$ (i.e., choose to move across the first row).

Our concern is only with the m^{th} and m-1^{st} power of λ. For $j \neq 1$, A_{ij} deletes both $(1+\alpha_{11}-\lambda)$ and $(1+\alpha_{jj}-\lambda)$, and λ does not appear in a_{1j}. Since each coefficient in A_{1j} appears only once in calculating the determinant, the maximum power of λ that can appear in all $a_{ij}A_{ij}$ terms, for $j \neq 1$, is λ^{m-2}. Therefore, we can ignore all terms in the expansion of cofactors but $a_{11}A_{11}$. To evaluate A_{11}, the matrix whose determinant is A_{11} must be expanded by cofactors repeatedly, until the remaining submatrix is 1 x 1 (thus, in principle, there will be $m-1$ expansions to compute $\det A$). Consider, then, the expansion of the matrix A with the first row and column deleted. By repetition of the above process we can look at:

$$\sum_{j=2}^{m} a_{2j}A_{2j}.$$

For the same reasons as above, for $j \neq 2$, A_{2j} deletes two λ entries, and we need only consider $a_{22}A_{22}$ to evaluate p_0 and p_1. That is, we only have to examine:

$$a_{11}(a_{22}A_{22}).$$

This process repeats itself, so that the third expansion eliminates all but:

$$a_{11}[a_{22}(a_{33}A_{33})].$$

That is, the process continues until the only term that can yield λ^m and λ^{m-1} is:

$$a_{11} \cdot a_{22} \cdot a_{33} \cdots a_{mm}, \text{ or}$$

$$\prod_{i=1}^{m} a_{ii} = \prod_{i=1}^{m} (1+\alpha_{ii}-\lambda).$$

The first step, then, has been shown. To evaluate the sign of λ^m and λ^{m-1}, we need to consider only the product of the elements on the main diagonal of A. The second step is to show that the signs of the two coefficients must be opposite.

By assumption, $\alpha_{ii} > 0$ for all i. Therefore, the sign of the p_0 and p_1 coefficients are uniquely determined by $(-\lambda)$, itself. Thus, if m is even, p_0 is positive, but p_1 is negative. If m is odd, p_0 is negative, but p_1 is positive. Consequently, p_0 and p_1 are necessarily of opposite sign, the Routhian necessary (and sufficient) conditions can not be maintained, and the cited theorems must be true. Since the proof does not depend on the size of m, Theorems 1 and 3 hold as special cases. Q.E.D.

The Two-Candidate Case

First, I will illustrate the derivation of Equation (12). Equation (11) is:

$$R_{it+1} = (1+\alpha_{11})R_{it} - \alpha_{12}R_{jt} + \alpha_i;$$

$$R_{jt+1} = -\alpha_{21}R_{it} + (1+\alpha_{22})R_{jt} + \alpha_j.$$

The general solution is obtained by determining the characteristic vector of the associated characteristic value problem. In particular, the following determinant is set to zero and solved for the two values of $\lambda^{(q)}$:

$$\begin{vmatrix} 1+\alpha_{11}-\lambda^{(q)} & -\alpha_{12} \\ -\alpha_{21} & 1+\alpha_{22}-\lambda^{(q)} \end{vmatrix} = 0.$$

Equation (12) then follows as the algebraic formula for the determinant.

Next, I will show, for $m = 2$, that the roots of the characteristic equation for the resource (and hence delegate) equations are real and, for the larger root, a positive number. From Equation (12), the roots of the characteristic equation will be imaginary (i.e., complex conjugates) only if the term in the radical is negative:

$$\alpha_{11}^2 + \alpha_{22}^2 - 2\alpha_{11}\alpha_{22} + 4\alpha_{11}\alpha_{21} < 0.$$

The first three terms can be factored to yield:

$$(\alpha_{11} - \alpha_{22})^2 + 4\alpha_{12}\alpha_{21} < 0.$$

The first term is obviously positive, and, since α_{12} and α_{21} are negative by assumptions, $4\alpha_{12}\alpha_{21}$ must be positive also. Therefore, the roots can not be complex conjugates. Second, since all terms outside the radical in Equation (12) are positive numbers, the larger of the two roots must be a positive number. (Note that Theorems 1 and 3 can be proven by setting Equation (12) to be less than one and showing that so doing leads to a contradiction).

To prove Theorem 2 (and thus Theorem 4) consider Equation (11). On substitution to make C_i's (C_j's) resources a function of only his or her own resources, and with suitable rearrangement, the following is obtained:

$$\begin{aligned} R_{it+2} &- (2+\alpha_{11}+\alpha_{22})R_{it+1} \\ &+ (1+\alpha_{11}+\alpha_{22}+\alpha_{11}\alpha_{22}-\alpha_{12}\alpha_{21})R_{it} \\ &= -\alpha_{22}\alpha_i - \alpha_{21}\alpha_j; \end{aligned}$$

$$\begin{aligned} R_{jt+2} &- (2+\alpha_{11}+\alpha_{22})R_{jt+1} \\ &+ (1+\alpha_{11}+\alpha_{22}+\alpha_{11}\alpha_{22}-\alpha_{12}\alpha_{21})R_{jt} \\ &= -\alpha_{11}\alpha_j - \alpha_{12}\alpha_i. \end{aligned}$$

This form is similar to those commonly encountered in economics and is an alternative form for solving simultaneous linear difference equations than used here (cf. Goldberg, 1958). More to the point, the two candidates' equations differ only in the constant term. Therefore, the dynamic properties, being dependent only on the coefficients of the variables (i.e., the left-hand side of these equations), are the same for the two candidates. Thus, Theorems 2 and 4 follow.

The "Argument" for Propositions 2 and 3

It appears that the veracity of Propositions 2 and 3 depends upon the magnitudes of the various coefficients involved. At present, I make no assumption about their sizes. However, I will show here the conditions under which the magnitude of a perturbation from a given value for the three-candidate case will be larger than the two-candidate case. By extension, the conditions for larger m are similar. Since these conditions appear likely to be met under most plausible values of the coefficients, I "argue" for the veracity of the two propositions. Propositions 1 and 4 are, of course, but "corollaries" of the proven theorems and the assumptions leading to Equations (4)–(6).

Suppose there is some value to the equations at a given time (denoted by *). One might think of this as an "equilibrium," i.e., a point of balance. However, it is unstable, and any perturbation will lead to deviations away from this point of balance. The question, then, is how rapid or large the deviations are. In particular, are they likely to be larger or smaller for $m = 3$ than $m = 2$? For the three-candidate case, the equations for C_i are:

$$DS_{it}^{*} = a_{11}R_{it}{}^{*} - a_{12}R_{jt}{}^{*} - a_{13}R_{kt}{}^{*} + c_i;$$

$$\begin{aligned} \Delta R_{ij}{}^{*} = b_{11}DS_{it}{}^{*} &- b_{12}DS_{jt}{}^{*} - b_{13}DS_{kt}{}^{*} \\ &+ d_t. \end{aligned}$$

Now, suppose there is some perturbation. For example, suppose $DS_{it}{}^{*}$ at some time t is incremented by a small positive amount, e_i, while the other two candidates just met expectations exactly.

If so, this perturbation will feed into the resources equations thus:

$$\begin{aligned} \Delta R_{it} = b_{11}(DS_{it}{}^{*} + e_i) &- b_{12}DS_{jt}{}^{*} \\ &- b_{13}DS_{kt}{}^{*} + c_i; \end{aligned}$$

$$\begin{aligned} \Delta R_{jt} = -b_{21}(DS_{it}{}^{*} + e_i) &+ b_{22}DS_{jt}{}^{*} \\ &- b_{23}DS_{kt}{}^{*} + c_j; \end{aligned}$$

$$\begin{aligned} \Delta R_{kt} = -b_{31}(DS_{it}{}^{*} + e_i) &- b_{32}DS_{jt}{}^{*} \\ &+ b_{33}DS_{kt}{}^{*} + c_k. \end{aligned}$$

That is, the effect of C_i's gain above expectations will affect all three candidates. If there were an equilibrium that is now perturbed, the difference between actual and "equilibrium" resource acquisition rates will be:

$$\Delta R_{it} - \Delta R_{it}^* = b_{11} e_i;$$

$$\Delta R_{jt} - \Delta R_{jt}^* = -b_{21} e_i;$$

$$\Delta R_{kt} - \Delta R_{kt}^* = -b_{31} e_i.$$

By simple algebra, it is apparent that terms such as $R_{it+1} - R_{it+1}^*$ will equal the right-hand side of the above equations as well.

These terms serve as input into DS_{it+1} equations. C_i is helped by the added increment to his or her resources and the loss suffered by opponents. C_j (and C_k) are helped by C_k (C_j) suffering a reduced rate of resource acquisition, but are hurt by C_i's gain and, of course, his or her own loss. In particular:

$$DS_{it+1} - DS_{it+1}^* = a_{11} b_{11} e_i + a_{12} b_{21} e_i + a_{13} b_{31} e_i;$$

$$DS_{jt+1} - DS_{jt+1}^* = -a_{21} b_{11} e_i - a_{22} b_{21} e_i + a_{23} b_{31} e_i;$$

$$DS_{kt+1} - DS_{kt+1}^* = -a_{31} b_{11} e_i + a_{32} b_{21} e_i - a_{33} b_{31} e_i.$$

That is, C_i will exceed expectations in the primary at time t+1 by the addition of all three terms, while the other two will be affected negatively by C_i's and their own terms, while "helped" by their other opponents' shortfall.

In more general terms, what we are doing is a study of the "qualitative matrix," i.e., the multiplication of matrices, examining only the signs of resultant terms. For the three candidate case, the effect of the initial perturbation can be shown as the result of multiplying the following matrices; the first that of the signs of the coefficient matrix, the second the matrix of disturbances:

$$\begin{bmatrix} + & - & - \\ - & + & - \\ - & - & + \end{bmatrix} \begin{bmatrix} + \\ 0 \\ 0 \end{bmatrix} = \begin{bmatrix} + \\ - \\ - \end{bmatrix} .$$

The right-hand side summarizes the effect on resources, i.e., $\Delta R_t - \Delta R_t^*$ and $R_{t+1} - R_{t+1}^*$. The next iteration yields the effect on $DS_{t+1} - DS_{t+1}^*$:

$$\begin{bmatrix} + & - & - \\ - & + & - \\ - & - & + \end{bmatrix} \begin{bmatrix} + \\ - \\ - \end{bmatrix} = \begin{bmatrix} (+ & + & +) \\ (- & - & +) \\ (- & + & -) \end{bmatrix} .$$

Repeating this procedure yields the qualitative impact on $R_{t+2} - R_{t+2}^*$, etc.

The effect of this perturbation, then, can be used to compare the two- and three-candidate cases. If we assume for the moment that all terms are of equal importance, we can summarize the *net* advantage for C_i, the favored candidate, simply by counting the number of positive terms for C_i and negative terms for an opponent or opponents and subtracting from that the number of negative terms for C_i and positive terms for C_j and, if relevant, C_k. Table 1 indicates the net balance for C_i. In support of the propositions, the three-candidate case does appear to have a greater impact, given the assumptions made in Table 1. It should be pointed out that the balance of effects would be even more striking if the "feedback" terms are larger than the "reaction" terms. That is, if how candidates fare is more dependent upon their own fortunes than on their competitors' (the on-diagonal term are larger than the off-diagonal terms), the apparently greater instability of the three-candidate case would be even more exaggerated. There is some evidence that, especially in larger fields of competition, the candidates are affected more directly by their own successes or failures (see Aldrich, 1980).

A second case is that one candidate has a positive perturbation, while one other obtains a negative one. For the three-candidate case, this example will provide the illustration of how the third candidate can be immune (at least temporarily) to the dynamic forces.

In the comparable table for the two- and three-candidate cases, each candidate is treated separately, since each has a unique number of positives and negatives (see Table 2). The entries are the number of positive terms for a candidate, less the number of negatives for himself or herself.

Obviously, the effect of this change in the example was, essentially, to "cancel out" C_k and make the deviations "like" that for the two-candidate contest. That is, there is no evidence of an effect of *m* in this case.

Finally, suppose that *both* C_j and C_k have a negative perturbation, then the advantage to C_i in this case can be compared to (just) a negative perturbation for C_j in the two-candidate case, with C_i having a positive effect in both in-

Table 1. The Effect of a Single Perturbation on Deviation from Equilibrium

	$R_{t+1} - R_{t+1*}$	$DS_{t+1} - DS_{t+1*}$	$R_{t+2} - R_{t+2*}$	$DS_{t+2} - DS_{t+2*}$
2 candidates	+2	+4	+ 8	+16
3 candidates	+3	+5	+11	+17

Source: Compiled by the author.

stances. The summary advantage for C_i is presented in Table 3.

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Table 2. The Effect of One Positive and One Negative Perturbation on Deviation from Equilibrium

	Perturbation	$R_{t+1} - R_{t+1*}$	$DS_{t+1} - DS_{t+1*}$	$R_{t+2} - R_{t+2*}$	$DS_{t+2} - DS_{t+2*}$
2 candidates					
C_i	+	+2	+4	+8	+16
C_j	–	–2	–4	–8	–16
3 candidates					
C_i	+	+2	+4	+8	+16
C_j	–	–2	–4	–8	–16
C_k	0	0	0	0	0

Source: Compiled by the author.

Table 3. The Effect of One Positive and Two Negative Perturbations on Deviation from Equilibrium

	$R_{t+1} - R_{t+1*}$	$DS_{t+1} - DS_{t+1*}$	$R_{t+2} - R_{t+2*}$	$DS_{t+2} - DS_{t+2*}$
2 candidates	+4	+ 8	+16	+32
3 candidates	+5	+11	+17	+43

Source: Compiled by the author.

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Self-Interest vs. Symbolic Politics in Policy Attitudes and Presidential Voting

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This article contrasts short-term self-interest and longstanding symbolic attitudes as determinants of (1) voters' attitudes toward government policy on four controversial issues (unemployment, national health insurance, busing, and law and order), and (2) issue voting concerning those policy areas. In general, we found the various self-interest measures to have very little effect in determining either policy preferences or voting behavior. In contrast, symbolic attitudes (liberal or conservative ideology, party identification, and racial prejudice) had major effects. Nor did self-interest play much of a role in creating "issue publics" that were particularly attentive to, informed about, or constrained in their attitudes about these specific policy issues. Conditions that might facilitate more self-interested political attitudes, specifically having privatistic (rather than public-regarding) personal values, perceiving the policy area as a major national problem, being high in political sophistication, perceiving the government as responsive, or having a sense of political efficacy, were also explored, but had no effect. The possibility that some long-term self-interest might be reflected in either group membership or in symbolic attitudes themselves is examined. While such possibilities cannot be definitively rejected, problems with interpreting standard demographic findings as self-interest effects are discussed.

In recent years, "rational" decision-making models have made a comeback in analyses of electoral behavior. Spurred on partly by V. O. Key's (1966) faith that "voters are not fools," political scientists have increasingly emphasized the cognitive, calculating, sensible, up-to-date, and utility-maximizing aspects of the voter's behavior. In the tradition of Downs' spatial theories, some analysts have emphasized "rational choice" models which presuppose a series of complex perceptions and calculations on the part of the voters, eventuating in support for the candidate or party closest to the voter on issues important in the campaign (e.g., Page, 1977; Riker and Ordeshook, 1973). That is, "issue proximity" determines the vote. Others have presupposed less elaborate cognitive activity, arguing that voters make sensible but more global retrospective evaluations of the incumbent's performance, and then reward or punish accordingly with their votes (Fiorina, 1978; Kramer, 1971; Tufte, 1978). Either way, an important corollary has been that such judgments affect electoral behavior disproportionately among those with a special interest in the policy area in question (that is, its unique "issue public"), rather than uniformly across issues and voters.

Much empirical evidence demonstrates the correlations of issue proximity and performance judgments with voting behavior, espe-

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cially within specifiable issue publics. But what, in turn, are the origins of these voter judgments? And what generates an "issue public?" In general, theories on this subject come mainly from economic theories of human behavior. These simply hold that voters try to maximize their perceived utilities, i.e., the worth or value of any given outcome to the individual. Such theories do not usually specify the nature of those utilities very precisely, but they almost always assume, at least implicitly, that the most powerful are those with tangible impact on the voter's personal life. In common-sense language, "self-interest" is the main driving force behind voters' decisions. Yet this assumption has rarely been made very explicit, nor tested empirically in any very precise or tenacious way. The empirical pursuit of evidence of such self-interest is, therefore, a main focus of this study.

Before we proceed, though, it would be wise to specify clearly the boundaries of the self-interest hypothesis. A self-interested attitude is usually defined as one that is instrumental to the individual's attainment of valued goals. However, to make the hypothesis falsifiable and non-trivial, we prefer to restrict the range of goals to those which bear directly on the material well-being of individuals' private lives, concerning their financial status, health, domicile, family's well-being, etc. Excluded, therefore, are such nonmaterial goals as spiritual well-being, moral rectitude, prestige or status, relief from neurotic anxiety, or altruism. Excluded also are very long-term calculations about self-interest that do not bear directly on the voter's well-being in the short run. Such restrictions align our notion of "self-interest" with such common terms as "selfish" or "greedy," emphasizing the egocentric, material, and short-to-medium term determinants of human behavior. While the excluded considerations all may exert important influences upon the formation of voters' attitudes, they involve less straightforward versions of the self-interest idea, and go well beyond the way ordinary people use the term. We will give them separate treatment once this more restricted version of self-interest has been dealt with.

The alternative point of view we wish to contrast with self-interest may be termed "symbolic politics" (Sears, Hensler, and Speer, 1979). By this line of thinking, people acquire stable affective preferences through conditioning in their preadult years, with little calculation of the future costs and benefits of these attitudes. The most important of these are presumably some rather general predispositions, such as party identification, liberal or conservative ideology, nationalism, or racial prejudice. When confronted with new policy issues later in life, people respond to these new attitude objects on the basis of cognitive consistency. The crucial variable would be the similarity of symbols posed by the policy issue to those of long-standing predispositions. Political attitudes, therefore, are formed mainly in congruence with long-standing values about society and the polity, rather than short-term instrumentalities for satisfaction of one's current private needs. In the world of "symbolic politics," one's political and personal lives exist largely isolated from one another.

The main purpose of this study is to test these two competing explanations for the formation of policy attitudes and for issue-based voting. Do people support those policies that further their private gains, or do policy preferences originate instead in political predispositions which are largely the residue of an earlier political socialization that was ignorant of present self-interest? And, do policy preferences affect voting behavior more when they are based in self-interest?

Surprisingly, the precise role of self-interest in determining voters' attitudes has received little empirical attention in the past, perhaps because it has seemed so straightforward. Ideally, it would require relating the direct personal impact of a policy issue to a voter's policy preferences. More often, indirect measures have been used—mainly macro-level data and demographic variables—and voters' self-interest inferred from them. However, in both these cases, the independent variables are so removed from the direct impact of a policy issue on the individual that they provide at best imprecise, ambiguous indicators of it. For example, macro-level fluctuations in the economy hurt the electoral outcomes of an incumbent president's party (Kramer, 1971), but there is apparently little connection between personal economic impact and electoral defection at the individual level (Kinder and Kiewiet, 1979). That is, self-interest had only weak effects.

Similarly, the personal impact of a policy issue clearly depends partly on the individual's social location; e.g., the urban poor are more likely to benefit from job training programs than are suburban professionals. Demographic variables are widely used to index personal impact (see Campbell et al., 1960, Ch. 9, among many others). But they prove to be excessively imprecise indices of this kind of self-interest. Most demographic variables do describe people's current social locations, and so, to some degree, the probable impact of any given government policy on them. But most also describe their preadult social backgrounds to some degree, and hence their socialization. For example, the strongest opposition to the Vietnam War, at least in its later stages, came from the young. This age effect might seem at first glance to have been based on the war's personal impact on them, since the young were most likely to be sent into combat. But other evidence suggests the age effect really reflected differential political socialization. Opposition to the war was greatest among young women, and young men in elite colleges, few of whom were very vulnerable to the draft. But their socialization had made them unsympathetic to violence, militarism, and virulent anticommunism. Age indexed not only the personal impact of the war, but differential political socialization as well. As a general matter, then, demographic variables are inadequate indicators of self-interest, because they inextricably confound it with socialization and therefore the origins of symbolic predispositions.

Such considerations argue for indexing self-interest more directly in terms of a policy's concrete impact on the individual's personal life. Some such efforts have found self-interest not to have major effects on policy attitudes and voting behavior (e.g., Kinder and Kiewiet, 1979; Sears et al., 1979). Instead, symbolic predispositions such as racial prejudice, liberal or conservative ideology, and party identification had the major effects. To be fully convincing, however, such analyses need to be supplemented with national studies on a wider range of policy issues, which the present study affords.

Our second main goal is to explore special conditions that might maximize the effects of self-interest on policy preferences, given the weakness of self-interest effects in prior studies. The condition most stressed in both rational-choice theories and empirical studies of public opinion is information; voters need to be able to figure out what policy position maximizes their self-interest (e.g., Converse, 1975). An abstract ideology might also be necessary to help guide the voter through the complexities of determining selfish policy preferences in an abstruse political system (Campbell et al., 1960, p. 209). Voters might respond more to self-interest, also, if they believed that solving a given problem is the government's responsibility, that the government is ready to act on it, and in response to public opinion, and to that citizen's opinion in particular. The voters may also need to believe that it is legitimate to act politically in a self-interested, private-regarding way, rather than in a more public-regarding manner (Wilson and Banfield, 1971). As well as these standing predispositions, certain causal attributions linking self-interest to the particular policy in question might be required. The most relevant would concern both the origins of the policy problem and who is responsible for its solution (Brody and Sniderman, 1977). The hypothesis would be that self-interest affects policy preferences only when voters perceive government action to solve the problem to be relevant and legitimate. Interactions of such variables with self-interest will be explored, to determine whether or not they are preconditions to self-interest effects.

Finally, self-interest may generate "issue publics." That segment of the public most concerned about a particular issue is likely to be marked by paying more attention to it, collecting more accurate information on candidates' positions about it, and having more consistent policy attitudes about it. All of these indicators of being in an "issue public" may be stimulated when the issue affects the voter personally. For example, Converse (1975) has argued that "doorstep issues" are especially likely to produce both attentiveness to an issue and attitudinal constraint. And Nie et al. (1976) argued that the well-known rise in issue consistency and issue voting in the late 1960s was partially due to increased personal impact of national issues. Lau et al. (1978) found only weak support for both expectations, but there is clearly need for a more extended analysis of the self-interest contribution to the generation of "issue publics."

The data for the study came from the 1976 CPS presidential election survey. That election is a particularly apt case because of the manifest relevance of self-interest issues (principally economic ones) to the presidential choice. Also, the 1976 CPS study contained an unusually fertile series of both self-interest and symbolic measures in four policy areas: unemployment, medical insurance, busing, and crime. It involved a nationally representative panel survey with two waves of interviews, one before and one after the election. Since some crucial variables were only asked in the post-election wave, only respondents interviewed in both waves are considered here. The weighted N is 2403.

Determinants of Policy Attitudes

The analysis begins by considering the determinants of preferences in the four policy areas mentioned, using regression analysis. The independent variables are (1) self-interest in (i.e., the personal impact on respondents of) the policy issue, and (2) symbolic attitudes. (3) Relevant demographic variables are also used, for better specification of the models, but since their theoretical status is ambiguous, they will be dealt with on a case-by-case basis. Three *symbolic attitudes* were used as appropriate: party identification, liberal or conservative ideology, and racial prejudice. The same five *demographic* variables were used in most analyses: age, education, family income, sex, and race. For the busing analysis, race was not used since only white respondents were considered. It was replaced by a dummy variable indexing whether the person was living in the South or not at age 14.

In each policy area, there were three or four indices of *self-interest.* For the unemployment policy issue, self-interest concerned whether or not (1) the respondent or other head of household was currently unemployed or temporarily laid off, (2) the family was worse off financially than a year earlier, and/or (3) the recession had hurt the respondent's or family's employment situation. For the national health insurance issue, self-interest was indexed by having (1) no current insurance coverage, (2) current insurance coverage too low to cover major medical costs, and/or (3) excessively costly insurance premiums. For busing, self-interested whites (1) had a child in public school, (2) did not have a child already riding a bus to school, (3) lived in a district with busing happening or rumored, and/or (4) lived in an all-white neighborhood. For crime, self-interested respondents (1) had recently been victimized by crime, (2) felt their own neighborhood was not safe to walk alone in at night, and/or (3) stayed away from certain parts of town because of fear of crime. In each case, a single *policy attitude* dependent variable was used. The details of all these variables and scale construction are presented in the appendix.

Generally speaking, we found self-interest to have little effect on voters' policy preferences, while symbolic attitudes had major effects. The simple regressions, in Table 1, show that no self-interest index significantly affected whites' opposition to busing; respondents personally affected by the recession were somewhat more favorable to government-guaranteed jobs; recent victims of crime showed somewhat greater support for law-and-order policies; and both those without medical insurance, and those perceiving their current coverage as inadequate, were more apt to support national health insurance. But only four of the thirteen self-interest indicators had even a statistically significant effect. The simple total contribution to R^2 of self-interest ranged between 3.1 and 0.4 percent across the four policy areas, as shown at the top of Table 2.

In contrast, all symbolic attitude variables had significant effects, and some were quite strong. In all cases, liberalism-conservatism far outstripped any of the self-interest variables, and racial prejudice did so in the two areas (busing and law and order) where it was relevant. The simple contribution of symbolic attitudes to R^2 ranged between 17.1 and 10.1 percent, as shown in Table 2.

More telling still is the contribution of each category once the other has been taken into account; i.e., the unique contribution to R^2 of each category of predictors. If we conduct a stagewise regression using the three categories of variables (symbolic attitudes, self-interest, and demographics) as the three steps, and vary the order of entry, we can assess the unique contribution of each category. Again, symbolic attitudes are consistently much more important than self-interest in determining policy preferences. Comparison of rows 4 and 5 in Table 2 shows that self-interest contributes, uniquely, from 0.4 to 2.4 percent, depending on the policy area, while symbolic attitudes contribute between 9.0 and 17.3 percent. Symbolic attitudes are always at least four times as powerful.

We treated the self-interest variables in several other ways, but none of the variations altered the basic outcome. Treating being unemployed and being laid off as separate variables, or narrowing the recession's effect to the respondent alone, did not change their effects on unemployment policy preferences. For attitudes toward national health insurance, we formed a composite index from the three self-interest items, which raised R^2 slightly from .147 to .162, but scarcely changed any individual terms (the composite drew a beta of .11). We ran the busing regression separately for people living in districts with busing and those where busing was rumored (in both of which children might genuinely be susceptible to being bused), but in neither case did the remaining self-interest items reach significance (though having a child in public school came close with rumored busing: $p < .10$). Also some of the measures of self-interest and policy attitudes were badly skewed on the busing issue, raising the possibility of underestimating true self-interest effects. Hence we redid that analysis, first using only a less skewed, general school integration item (v3211), and then just among self-proclaimed liberals and the least-prejudiced third of the sample (whose busing attitudes were not so skewed), but the findings remained almost exactly the same. Distributions of all variables on the other issues were reasonably balanced. In brief, the basic finding is that symbolic attitudes are the main predictors of respondents' policy attitudes; self-interest contributes only trivially.

Special Conditions for Self-Interest Effects. Self-interest may only operate under certain circumstances, however, and so may be strongest in interaction with other variables. We therefore tested its interactions with five predispositions: political sophistication, private-regarding values, perceived government responsiveness, sense of political efficacy, and perceiving the issue as a very important problem. Measurement of these variables is described in the appendix. The analysis involved splitting the respondents at the median on each of these variables in turn, and then rerunning the basic regressions shown in Table 1 on these subsamples separately. Without going into unnecessary detail, we can say that none of these additional variables made any appreciable difference in the effects of self-interest on policy preferences. Indeed, in general, the overall R^2 for the equations, and the magnitude of the self-interest, symbolic, and demographic effects, were virtually identical for respondents high or low in these potentially moderating variables. In a separate analysis, examining only the most efficacious 20 percent (rather than the top 50 percent) of the sample did not materially improve matters.

Sophistication is perhaps the most theoretically interesting of these, because "rational choice" presumably depends upon adequate information. Sophistication did generally elevate the predictability of these equations (R^2 increased by an average of 7.5 percent). But it did not affect self-interest at all: seven of the thirteen self-interest terms (considering all issue areas) yielded higher unstandardized regression coefficients in the more sophisticated half than in the less sophisticated half of the population. Four were statistically significant in each group. Sophistication did seem to strengthen symbolic attitudes somewhat: seven of the nine terms were larger with high sophistication. This contrast can be seen more systematically in the analysis shown in Table 3. Sophistication strengthens the unique contribution of sym-

Table 1. Predictors of Support for Specific Policies

	Guaranteed Jobs	National Health Insurance	Busing (Whites only)	Law and Order
Self-interest Index[a] (yes)				
1	.01	.11***	–.06	.06**
2	–.01	.10***	–.01	.00
3	.06**	.03	–.02	.04
4	–	–	.01	–
Symbolic attitudes				
Liberal-conservative ideology (liberal)	.21***	.25***	.13***	–.22***
Party identification (Democratic)	.08***	.10***	–	–.05*
Racial prejudice (tolerant)	–	–	.31***	–.25***
Demographics				
Sex (male)	–.07***	.01	.03	.19***
Age (years)	.01	.06**	.00	.07**
Education (years)	–.08***	.01	.07**	–.01
Income ($)	–.11***	–.11***	–.06*	.06**
Race (white)	–.23***	–.07	–	.05*
Southern origin	–	–	.02	–
R^2	.215	.147	.150	.221
N	1965	1727	1562	1955

Source: Computed from data collected in the 1976 election study, Center for Political Studies, University of Michigan.

Note: Entries are betas.

[a] See appendix for a description.

*$p < .05$
**$p < .01$
***$p < .001$

Table 2. Variance in Policy Attitudes Accounted for by Each Category of Predictor Variables

Contribution of:	Guaranteed Jobs	National Health Insurance	Busing (Whites only)	Law and Order
Self-interest	.031	.027	.006	.004
Symbolic attitudes	.108	.101	.140	.171
Demographics	.156	.055	.029	.097
Symbolic attitudes beyond self-interest	.090	.098	.138	.173
Self-interest beyond symbolic attitudes	.013	.024	.004	.007
Demographics beyond symbolic attitudes	.104	.029	.006	.046
Demographics beyond self-interest	.130	.045	.029	.101
Total R^2	.215	.147	.150	.230

Source: Computed from data collected in the 1976 election study, Center for Political Studies, University of Michigan.

Note: The amount of variance explained is an indication of how "useful" each set of predictor variables is in predicting variance in the policy attitudes. When these predictors are correlated, the amount of variance any category predicts when considered alone overestimates that category's unique contribution to the regression. But the increase in R^2 produced by adding that category to a stagewise regression after another category has already been included provides an index of its unique contribution to variance explained. To give an example of how these usefulness figures are computed, self-interest and symbolic attitudes together explain .121 of the variance in attitudes toward guaranteed jobs. When the contribution of self-interest alone (.031) is subtracted from this, the remainder (.090) is the amount of additional variance explained by symbolic attitudes above and beyond that explained by self-interest. The data for this table comes from the regressions shown in Table 1.

bolic attitudes (rows 1 and 2) but not of the self-interest indices (rows 3 and 4).

Nor did having a clear political ideology make people more selfish. To test this, we compared the effects of self-interest in the unemployment issue among people who indicated strong ideological convictions by responding to all four liberal-conservative items (see the appendix) to those with little ideology (those who responded to only one). The stronger ideologues actually generated lower b-weights for self-interest on two of the three items (though not by much). So presence of ideology appears not to facilitate self-interest, either.

Finally, the voters' self-interest may affect

Table 3. Variance in Policy Attitudes Accounted for by Each Category of Predictor Variables, Controlling on Sophistication

Contribution of:	Guaranteed Jobs	National Health Insurance	Busing (Whites Only)	Law and Order
Symbolic attitudes beyond self-interest				
High sophistication	.112	.172	.163	.222
Low sophistication	.068	.029	.107	.124
Self-interest beyond symbolic attitudes				
High sophistication	.009	.032	.008	.008
Low sophistication	.039	.016	.010	.009
Demographics beyond self-interest and symbolic attitudes				
High sophistication	.060	.013	.008	.050
Low sophistication	.125	.017	.016	.036

Source: Computed from data collected in the 1976 election study, Center for Political Studies, University of Michigan.

Note: The data for this table are based on regressions identical to those presented in Table 1, but done separately for respondents high and low in sophistication. Entry is R^2, computed as in Table 2.

their policy preferences only when they make the right attributions about the origins of the policy problem and locus of responsibility for its solution. Self-interest should influence preferences about government policy only when voters attribute their problems to causes external to themselves, and/or feel the government is responsible for their solution. If one perceives one's own plight to be caused by one's situation, rather than by one's own actions, then one should regard government action to change the situation as most appropriate. For the economic policy areas, we generated scales to measure perceived locus of causality for poverty (whether the poor, or their situation, is to blame), and perceived government economic responsibility (whether or not it is up to government to solve various economic problems). (See the appendix for details.) However, when we ran the basic regression on the unemployment issue (Table 1) only among those above the median in external attributions, all self-interest terms washed out. In the case of attitudes about medical insurance, we found two of the self-interest terms to be stronger among respondents with external locus of causality, and one weaker. No relevant attributions were available for the other two areas. There is little evidence, then, that appropriate causal attributions increased the power of self-interest.[1]

Alternative Models

In testing for respondents' self-interest, we have initially used only direct measures of the impact of policy issues on their personal lives. However, some of our other predictors may also be picking up the effects of personal impact, perhaps yielding some genuine self-interest effects we are overlooking.

Symbolic Attitudes. One possibility is that the symbolic attitudes themselves reflect self-interest, rather than some archaic relics of a childhood socialization process blind to the child's ultimate adult interests. Such predispositions may be constantly reformulated to capture and synopsize a variety of the voter's interests and provide cognitively simple and economical cues to self-interest amid the complexities of changing issues and candidates. One version of this notion is that symbolic attitudes are reasonably responsive to the personal impact of government policy over the short-to-medium term. This can be addressed with the data here. Another is that they are adjusted in an interest-maximizing manner over many years and in relation to many issues; we will return to this later on.

At the least, the voters' symbolic attitudes seem clearly not to express the short-term personal impact of policy issues. The correlations between the self-interest variables and symbolic attitudes are miniscule. On the unemployment issue, the median correlation between the three self-interest measures and the two symbolic dimensions was +.08; regarding national health insurance, none of the six correlations exceeded +.05; for busing, none of the eight correlations exceeded +.11, with a median of +.04; for crime, the nine correlations ranged between +.03 and –.13, with a median of –.03. These negligible correlations replicate earlier findings that whites' symbolic attitudes were unaffected by the personal impact of the race issue (Kinder and Sears, 1980; Sears et al., 1979).

A more rigorous test of this alternative exploits the CPS 1972–1976 panel study. If symbolic attitudes are constantly readjusted to express current self-interest, measures of symbolic attitudes taken in earlier years ought to be of diminished value in predicting to current policy attitudes (since they could not so accurately reflect current self-interest). Current self-interest and its surrogate, current symbolic attitudes, ought by contrast to have much better predictive value. However, including 1974 symbolic measures in regressions which otherwise use 1976 data weakens the influence of self-interest still further, and actually strengthens symbolic attitudes. On the unemployment issue, the simple contribution to R^2 of self-interest drops to 1.9 percent, and its unique contribution to 1.2 percent. In contrast, the contribution of symbolic attitudes increases to 12.3 and 11.6 percent, respectively. So it is very unlikely that current personal impact of an issue has any important indirect effect on voters' policy preferences through symbolic attitudes.

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[1] This phenomenological approach assumes such attributions are not caused by self-interest, symbolic attitudes, or the policy attitudes themselves. This is to us a dubious assumption, since causal attributions are well-known vehicles for rationalizing affective states (Bradley, 1978). A "symbolic politics" perspective would therefore treat them affectively, as symbolic attitudes (e.g., blaming the poor for their own poverty is simply another manifestation of conservatism). And indeed they have strong main effects when added to the regressions shown in Table 1, reducing the effects of liberal or conservative ideology somewhat, but increasing the overall R^2 by about 40 percent in each case.

Demographic Variables. A second possibility is that demographic variables also index self-interest, since they too partially describe the individual's social location. Again there are two versions of this alternative, one short-term and one longer-term. The former simply suggests that the individual's social location, as indexed by demographic variables, partially determines the immediate personal impact of government policy, and therefore ought to be interpreted in self-interest terms. We can address this version here with our data. Demographic variables could also index self-interest in the longer-term, "ideology-by-proxy" sense suggested by Campbell et al. (1960, Ch. 10): poorly informed voters may cut their information costs by conforming to the political norms of groups with whom they perceive common fate. Conformity spares them the information search and calculations they would otherwise engage in by themselves. We will return to this version later.

If demographic variables had some significant effect upon voters' policy attitudes, above and beyond that contributed by our direct measures of personal impact, they might be tapping some unmeasured policy impacts. However, as we noted earlier, demographic measures also describe social background, and hence the socialization origins of symbolic attitudes. They cannot therefore be interpreted unequivocally as reflecting the personal impact of the policy issue; they rather represent some combination of it and symbolic attitudes. The mixture might seem hopelessly impenetrable, but it can be unscrambled to some degree by determining whether or not demographic variables contribute unique variance to policy attitudes, above and beyond that contributed by our direct measures of self-interest and symbolic attitudes. If they do not, it is unlikely that they constitute some untapped reservoir of self-interest variance.

In fact, demographic variables had rather weak effects in our data. Moreover, what variance they did contribute tended to overlap with that contributed by symbolic attitudes. So they probably mostly reflect the residual effects of earlier socialization rather than current self-interest. This point is clearest in the cases of medical insurance, busing, and crime, so let us consider them first. Here the demographics added relatively little variance to that contributed by symbolic attitudes (between 0.6 and 4.6 percent; see Table 2, row 6). They explained more variance above and beyond self-interest (between 2.9 and 10.1 percent; Table 2, row 7). Demographic factors have the strongest effects on the crime issue, so it merits a little fuller explication. White, male, and high-income respondents were the harshest law-and-order advocates, as shown in Table 1. But these cannot be disguised self-interest effects, because these same groups were the least frequently victimized by or vulnerable to crime. For example, women indicated more vulnerability than men by being more afraid to go out in their neighborhoods alone at night ($r=.29$). The alternative case, for socialization origins of law-and-order sentiments in these groups, is fairly clear; certainly males, and whites, are generally socialized to more punitive, authoritarian stances than are women and blacks.

The unemployment issue seems at first glance to yield the most plausible case that demographic variables are picking up self-interest effects. Table 1 shows that support for guaranteed jobs comes disproportionately from the groups most likely to profit from them: blacks, women, and those with low income. But the demographic variance is not shared with the direct measures of self-interest. When the demographic variables are omitted from the basic regression (Table 1, column 1), the R^2 drops (from .215 to .121) and the self-interest terms do not become much stronger (the beta for recession impact rises from .06 to .10, but the other two remain trivial). Similarly, the demographic variables lose little of their power when self-interest is added to the equation (the contribution of R^2 dropping only from 15.6 to 13.0 percent; compare rows 3 and 7 in Table 2).

So demographic variables contributed to support for guaranteed jobs independent of the variance supplied by the direct personal impact of employment problems. It is conceivable that in this case demographic factors are picking up effects of the current personal impact of the employment issue that are inadequately indexed by our more direct measures of self-interest. But it seems unlikely since the latter have considerable face validity, and seem to cover adequately the main employment problems people have. If this finding reflects some effect of self-interest, then, it seems more likely to require an expanded definition of self-interest; we will return to this point later.

1976 Presidential Voting

The next question was whether or not self-interest would affect issue voting. Specifically, simple self-interest theories would predict that policy attitudes would affect presidential voting more among those with some private interest in the issue than among those with none. That is, in predicting the vote, we would expect policy attitudes and self-interest to interact. The symbolic politics hypothesis, on the other hand, suggested that issue voting would occur without reference to self-interest; that is, that symbolic and policy attitudes would have main effects upon the vote, and not interact with self-interest.

A fully specified voting model would, of course, be based on a "funnel of causality," which passes from demographic variables through party identification to partisan attitudes concerning party, candidates, and issues (e.g., Campbell et al., 1960; Miller et al., 1976). Simply to assess the role of self-interest does not require a full account of presidential voting in 1976, however. We need to include the major variables, and then determine the marginal additional effects of self-interest. Hence, we specified regression equations in each policy area using a common set of symbolic attitudes and demographic variables, but each having a different "relevant policy attitude" (which was the dependent variable for the previous analyses), and different self-interest indices (as spelled out in previous sections). In addition, we included more general issue proximity indicators in three areas: economic issues, social issues, and busing.[2]

This model departs from the standard Michigan treatment in three ways. All variables are included in the equation simultaneously, rather than in the order implied by the "funnel," because we are not concerned with making precise distinctions among the contributions of other variables in the model. Liberal or conservative ideology is treated as a symbolic attitude analogous to party identification, rather than as simply one among a number of policy issues, since we consider it a generalized stable predisposition. Finally, we have ignored candidate evaluation, because it seems almost impossible to imagine it as unequivocally prior, causally, to candidate preference as expressed in the vote, when both are measured simultaneously. To repeat, we think these departures are not consequential for our main purpose, which is to estimate the proximal effects of self-interest above and beyond the effects of other variables.

This analysis generated terms for thirteen two-way interactions of self-interest and policy attitudes, considering the four issues. Of these, only two were significant. Persons not covered by medical insurance did vote more for the candidate whose positions were closest to theirs on medical insurance, but people *least* economically harmed by the recession voted more for the candidate whose views they agreed with most on guaranteed jobs. Only one of the thirteen self-interest indices had a significant main effect on the vote: declines in family financial situation were related to voting for Carter. The busing analysis was repeated within each of the critical "busing in area" subgroups (those for whom busing was happening or rumored in the area) but this change still did not yield significant interactions with self-interest.

It is hardly surprising that far and away the most important contributors to the vote are symbolic attitudes, especially party identification. Considering each category of variables by itself (as the first step in the stagewise regression, exactly as done earlier with policy attitudes in Table 2), symbolic attitudes and policy attitudes are most important (Table 4, rows 1–4). Symbolic attitudes retain that premier status even when self-interest and policy attitudes are considered (rows 5 and 6). Symbolic attitudes always add at least 20 percent to the R^2, even when other variables are considered first. But self-interest, policy attitudes, and demographic variables all lose their force when symbolic attitudes are considered first (rows 7–10). They never add more than 4 percent to the R^2 once symbolic attitudes have been taken into consideration. So in voting behavior, as in the generation of policy attitudes, symbolic attitudes have the greatest impact, and short-term self-interest apparently has almost no effect.[3]

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[2] It might also be noted that we recognize regression is often replaced by logit or probit analysis with dichotomous dependent variables. The disadvantages of regression analysis in such instances are serious chiefly with skewed data, and our dependent variable (the Carter-Ford division of the vote) is quite evenly distributed.

[3] This analysis may somewhat overestimate symbolic effects, by omitting candidate evaluations from the equation, or underestimate them, by not assessing their indirect effects through issues and candidate evaluations. To make precise allocations of the causal roles of these several factors demands a different research design from that of the 1976 CPS survey, we believe, especially obtaining measures at much earlier stages of the campaign. Our assumption, that party identification is causally prior to 1976 policy attitudes, can be checked, in part, by using 1974 or 1972 party identification on panel respondents. When we did this on the one issue (guaranteed jobs) yielding a significant self-interest effect, we found no changes in the role of self-interest, either as main effects or in interaction with policy attitudes on guaranteed jobs.

Issue Publics

Does self-interest create "issue publics?" That is, do voters with a greater personal stake in an issue perceive it as more salient, perceive candidates' positions on it more accurately, and have more consistent policy preferences about it? These data yield little evidence for such an effect. Mentions of an issue area as a major national problem indexed issue salience. Unemployed respondents did tend to mention employment as a major national problem more than did other respondents: 71 percent of those either unemployed or laid off spoke of unemployment as an important national problem, while only 56 percent of the employed did. Being affected by the recession had a weaker but still significant effect on issue salience, while personal financial reverses did not. The other three policy areas were not mentioned often enough as major national problems to warrant extending this analysis to them. None of the self-interest indices on any issue significantly improved the accuracy of perceptions of candidates' positions on the issue; indeed, the differences were never more than 5 percent, and most were in the wrong direction (i.e., the self-interested were less accurate). Finally, issue constraint (that is, the consistency of policy preferences) could be tested for on two issues. The unemployment policy item was asked both in the pre-election and in the post-election wave. Being unemployed or laid off increased the test-retest correlation for this policy preference (from $r=.50$ to $r=.61$), but being affected by the recession, or having suffered financial reverses, actually reduced it. On the busing issue, we assessed constraint using the correlations among the three items in the busing scale. These correlations were higher among the self-interested than the disinterested in eight of fifteen cases, and lower in the remaining seven. Indeed, the correlations are amazingly parallel. So there is little evidence here that voters' self-interest plays much of a role in creating "issue publics" for national issues.

Discussion

The main goal of this article has been to assess the relative roles of self-interest and symbolic attitudes in producing policy attitudes and issue voting on four controversial policy areas (unemployment, national health insurance, busing of school children for racial integration, and crime). In general, symbolic attitudes (liberalism-conservatism, party identification, and racial prejudice) had strong effects, while self-interest had almost none. A secondary goal was to explore some conditions that might facilitate more self-interested political behavior, but none was discovered that substantially affected the generally minimal level of self-interest effects (though sophistication did considerably increase the power of symbolic attitudes). Demographic variables generally had little effect beyond what was contributed by the more proximal symbolic attitudes

Table 4. Variance in 1976 Presidential Vote Accounted for by Categories of Predictor Variables

Contribution of:	Guaranteed Jobs	National Health Insurance	Busing (Whites Only)	Law and Order
Demographics	.109	.106	.047	.103
Self-interest	.135	.053	.058	.038
Policy attitudes	.216	.208	.189	.207
Symbolic attitudes	.390	.393	.370	.409
Symbolic attitudes beyond self-interest	.276	.350	.327	.375
Symbolic attitudes beyond policy attitudes	.211	.221	.219	.233
Policy attitudes beyond symbolic attitudes	.037	.036	.038	.031
Self-interest beyond symbolic attitudes	.021	.010	.015	.004
Self-interest beyond symbolic and policy attitudes	.009	.004	.006	.004
Demographics beyond symbolic and policy attitudes	.009	.010	.007	.014

Source: Computed from data collected in the election study, 1976 Center for Political Studies, University of Michigan.

Note: Entry is R^2, computed as in Table 2.

or self-interest measures (except in the case of unemployment), and so were unlikely to carry otherwise hidden self-interest effects.

A number of methodological complaints might be made about these analyses, but it can be said with some confidence that none we can think of accounts for the results. When composite self-interest measures, which are less skewed, are used, they usually generate somewhat stronger results, but of the same small order of magnitude. When the symbolic measures are dichotomized or trichotomized, to reduce variance, they have about the same effects. The same results occur when the policy attitudes are normally distributed (e.g., on crime) as when they are more skewed (e.g., on busing). Moreover, the few significant self-interest effects that did emerge fit no obvious pattern in terms, for example, of being subjective, objective, retrospective, or prospective.

A number of earlier studies have found self-interest to have little effect on policy attitudes (and voting behavior) on such issues as the economy, civil rights, the Vietnam War, busing, and the energy crisis (Bauer et al., 1963; Fiorina, 1978; Gatlin et al., 1979; Kinder and Kiewiet, 1979; Sears et al., 1978; Sears et al., 1979). There are some exceptions, however. Home ownership and being employed in the public sector had strong effects upon Californians' voting on property tax reform (Proposition 13) in 1978 (Sears, 1978), and the prospect of a local busing plan in Los Angeles was most opposed by parents likely to be affected by it (Allen and Sears, 1978). In the latter two cases the personal impact seems likely to have been considerably more definite, concrete, and immediate than is the case for most political issues, and this may be the reason that voters overcame the seemingly more common tendency to segregate political attitudes from their personal lives. In any event, symbolic attitudes also had powerful effects in both these cases. Hence, in our judgment, the present findings are quite robust; the concrete personal impact of political issues seems rarely to influence policy preferences and voting behavior very strongly.[4] The exceptions seem to involve unusual extremes of impact.

This extensive set of findings ought to shed some doubt on a central assumption of at least the more naive versions of economic or rational-choice theories. Indeed, it raises the question of why the self-interest assumption has been held so tenaciously yet apparently so uncritically. Part of the reason may be that it follows from one of the most common lay theories of human nature—a simple hedonic pleasure-pain theory—and so it seems "obvious." Partly, as we indicated earlier, the only self-interest indices in common use have been both quite indirect and subject to numerous alternative explanations: macro-level data, demographic variables, or group references in open-ended items (e.g., Campbell et al., 1960, pp. 203–40). In the absence of direct measures, self-interest had to be inferred from data that prove to have been misleading.

But we also need to consider the possibility that this apparent disconfirmation arises simply because our definition of self-interest was too restrictive. It will be recalled that it was defined in terms of the rather short-term, concrete impact of policy issues upon the individual's material well-being. A looser definition might open up some more impressive effects.

One such possibility is that "symbolic attitudes" themselves express some sense of self-interest which develops over a longer period, and therefore takes into account a broader collection of the person's interests. To be sure, other research that attributes symbolic attitudes to childhood socialization would seem to shed some doubt on this possibility (e.g., Sears, 1975). The child's political sophistication is so low that self-interest cannot be a major force at the time symbolic attitudes are initially acquired. And symbolic attitudes seem to be rather stable in later years; party identification and racial prejudice, for example, show virtually no change over four-year time spans (Converse, 1975; Sears, 1980). They are certainly insensitive to the short-term, concrete impact of policy issues upon the individual's personal life, as we have shown above.

Still, the evidence is by no means definitive. There is little direct longitudinal evidence on the long-term stability of symbolic attitudes, and even less on their responsiveness to changes in the individual's personal circumstances. Fiorina's (1978) painstaking analysis of the 1972–74–76 panel study data was able to uncover some evidence of small, only marginally significant modifications of party identification due to personal financial situation. But it is still possible that such predispositions do reflect long-term self-interest in some important way, perhaps cumulating and averaging over many issues and time periods. What we can say with some certainty is that both symbolic attitudes and policy preferences are remarkably indifferent to the individual's current personal situation, even when it involves such very striking phenomena as personal unemployment, the threat of catastrophic medical expenses, or having one's own child bused to distant ghetto schools. It may be that such personal events must be very severe, or cumulate over a longer period, to affect symbolic attitudes and policy preferences; our data do not scan such extreme cases adequately enough to be very informative on that point.

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[4] Another apparent exception is Tufte's (1978) use of 1976 NBC voter-exit surveys, which obtained strong correlations of personal financial situation with vote preference. Our suspicion is that this reflects the strong pressures to rationalize one's vote decision inherent in the voter-exit site, where the interviewer is essentially asking the voter for a socially acceptable explanation for his or her just-completed vote.

A second possibility is that self-interest operates indirectly through perceived shared group interests. By this argument, unsophisticated voters, having difficulty calculating what political posture is interest-maximizing, support the political positions taken by some group whose outcomes in private life seem over the long term to be correlated with their own. If this long-term self-interest process were operating, direct measures of immediate impact might not predict to policy preferences (voters finding it too complicated to figure out the maximizing policy stance) but group membership would (so they let the leadership figure it out). Campbell et al. (1960) term this process "ideology-by-proxy," but it is perhaps more accurately described as "self-interest-by-proxy."

Two bits of evidence in this study do suggest some self-interest might be operating in this "self-interest by proxy" manner. As indicated earlier, the substantial demographic effects on opinions concerning unemployment policy held irrespective of people's self-interest. Hence, voters may have used group membership as a guide to policy preference, while disregarding their own personal short-term self-interest. And there was some tendency for demographic variables to have increasing influence over the policy preferences of voters with lower levels of political sophistication (see Table 3, rows 5 and 6).

However, the case is not closed. These effects are not strong ones: they really held clearly only on the unemployment issue, of the four tested. And the unemployment issue would seem to be one on which a person's self-interest is *least* complicated to convert into political preferences (indeed, Campbell et al., 1960, p. 205, use it as the premier example of an issue so simple that the self-interest-by-proxy process is unnecessary). Moreover, in the absence of additional information, such demographic effects are just as plausibly interpreted as reflecting fraternal solidarity. Identification with fellow group members and empathic concern for their welfare, rather than indirect self-interest may be responsible for them.[5] And fraternal solidarity comes closer conceptually to our notion of symbolic politics than it does to self-interest.

Both of these latter possibilities require defining self-interest in terms of a somewhat longer time frame than we have been using. Obviously, our data cannot test for either very decisively. Whether or not more appropriate data would yield major self-interest effects cannot be anticipated for certain. All this discussion can hope to accomplish is to point up some of the problems in interpreting standard findings (mainly involving demographic factors) as documenting self-interest effects, even of the longer-term variety discussed here. It is to be hoped that more pointed tests are in the offing.

These findings do not settle *why* people derive their policy preferences from such general "symbolic attitudes," while ignoring the direct personal impact of government policy. From our perspective, the most plausible interpretation rests in "symbolic politics"; i.e., in rather unthinking, reflexive, affective responses to remote attitude objects. Affective responses are conditioned to symbols which, when salient, evoke consistent evaluations. We have no data directly assessing this process, however, and simply appeal to the vast literatures on the preadult acquisition of partisanship, social values, and racial prejudice, on the one hand, and on cognitive consistency pressures on the other, for evidence of it.

Alternatively, symbolic attitudes may express the adult's sense of the public good, and are quite deliberately and self-consciously given more weight than private considerations when voters make judgments about public policy. Perhaps political socialization teaches people to weigh most heavily the collective good when they don their "political hats," and to weigh their private good most heavily only when dealing with their personal affairs. To us, this too is plausible, and poorly tested with the few public- versus private-regarding value items used here.

Either way, the present findings suggest some modifications in the conventional view of political belief systems in the mass public. Most theorists have assumed that private concerns are central to political belief systems, and that they therefore constrain political attitudes.[6] This assumption has held across quite a variety of private concerns, including the individual's emotional life (McClosky, 1959), social relationships (Lane, 1962), economic and practical life (Downs, 1957), "doorstep issues" (Converse, 1975), or matters of class and status (Lipset, 1960). The "symbolic politics" viewpoint sees this quite differently, however. The central elements in political belief systems may be strong affective commitments to certain symbols, which remain constant for many years due to long histories of reinforcement. And they may constrain the individual's political responses to numerous other stimuli, such as policy issues, political events, media presentations, or electoral candidacies. But this provides no guarantee that they are central in other aspects of the individual's functioning. And the individual's private life may be quite peripheral to his or her political belief system, and both may be mutually quite unconstraining. For example, inconsistencies between expressing high levels of prejudice at a symbolic level, and displaying tolerant behavior toward minority individuals in private life, are common and well documented (Schuman and Johnson, 1976). Too little is known about the conditions under which private concerns influence people's attitudes about public affairs. But at the moment it appears these conditions are more limited than once thought.

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[5] Indeed, if the literature on relative deprivation offers a parallel, it may be even more plausible. There is some evidence that fraternal deprivation is more potent politically than is egoistic deprivation (Runciman, 1966; Vanneman and Pettigrew, 1972).

A common view today is that the practicing politician needs to pander to the citizenry's short-term, material, personal self-interest, because issue publics are composed of the relatively narrow subgroups who are directly affected by the issue in question. Indeed, "single-issue" issue publics are supposed to be focused quite intensely on some selfish concerns. The present data suggest instead that an "issue public" is comprised principally of those with shared symbolic loyalties, and that, similarly, a disaffected member of the "coalition of minorities" is one whose symbolic attitudes have been violated. The dangers to the politician practicing in the remote, largely media-conveyed world of public symbols perhaps lie, therefore, not so much in inconveniencing people in their private lives, as in violating their symbolic attitudes.

Appendix

The following lists the major variables used in these analyses and their designated variable numbers in the 1976 CPS codebook. In all cases where scales were used, the individual items were standardized and then averaged; half or fewer missing values were allowed.

Symbolic Attitudes: Party Identification: Standard 7-point scale (v3174). *Liberalism-Conservatism:* Scale based on 7-point liberal-conservative item (v3286), 10-point left-right item (v3360), and the difference between "feeling thermometer" ratings of conservatives and liberals (v-3838–v3823). *Racial prejudice:* Scale based on whether or not civil rights people are pushing too fast (v3213), whether or not whites have the right to keep blacks out of their neighborhoods (v3214), whether segregation or desegregation was favored in general (v3217), and feeling thermometer measures on blacks (v3832) and black militants (v3841) (these items were selected from a factor analysis of 13 race-related items, on which these five loaded between .27 and .63 on the first factor, while all others loaded .22 or below).

Demographic variables: Age (v3369), *Education* (v3384), *Family income* (v3509), *Sex* (v3512), *Race* (v3513), and *Region of origin* (a dummy variable, South or non-South at age 14: v3501).

Self-Interest: Employment: (1) self or head of household unemployed or laid-off (v3431); (2) self and family worse off financially (v3137); (3) scale based on effects of recession on respondent or respondent's family in terms of unemployment (v3146), reduced hours (v3148), pay cut (v3150), or additional reliance on governmental assistance (v3152). *Health Insurance:* (1) absence of personal health insurance coverage (v3281); (2) inadequate coverage for major illness (v3285); (3) excessively costly coverage (v3283). *Busing:* (1) having a child in public school (v3381); (2) not having child who already rides a bus to school (v3382); (3) living in an area with busing happening (v3209) or rumored (v3208); (4) living in an all-white neighborhood (v3221). *Crime:* scale based on (1) four recent victimization experiences: having witnessed a crime (v3919); having car (v3921), home or apartment (v3920) broken into; having been forcefully attacked (v3922); (2) not feeling safe walking alone in the neighborhood at night (v3910); (3) staying away from certain parts of town because of fear of crime (v3918).

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[6] Converse's (1975) speculations that ideology is most central for elites, and "doorstep issues" for the less sophisticated, are only half supported. Table 3 bears out the first, but provides no support at all for the second. Nor does it support the notion that sophistication increases the influence of self-interest on policy preferences (Campbell et al., 1960, p. 209).

Mediators: Sophistication: scale based on (1) education, (2) two political knowledge items, on which party controlled Congress (v3683 and v3684), (3) interest in the campaign (v3031), and (4) the number of media in which the campaign was followed (v3600, 3602, 3604, 3645). *Privatistic values:* scale based on rankings of five Rokeach instrumental and terminal values, keying positively "ambitious, hard working aspiring" (v3936), "prosperous life—good income and being able to afford the good things of life" (v3945), and "independent—self reliant, self-sufficient" (v3938); keyed negatively were "helpful, working for the benefit of others" (v3939) and being "responsible, dependable, reliable" (v3940). *Government responsiveness* and *political efficacy:* scales based on standard items (v3741–3744 and v3815–3818, respectively). *Major national problem:* (v3685–7) respondents could mention up to three issues; any of these three responses was counted for our purposes. Too few respondents mentioned anything about national health insurance (n=71), busing (n=25), or crime (n=83) to make separate analysis meaningful in these cases, but 1073 mentioned something about unemployment or jobs.

Attributions: Causes of poverty: Scale based on the six of seven available items which loaded together in a factor analysis: attributing poverty to the wealthy and powerful (v3751), too few jobs (v3752), too little chance for a good education (v3753), the seniority system in companies (v3754), inability to get into skilled unions (v3755), and our way of life, which doesn't give the poor an equal chance (v3757). Up to two missing values were allowed. *Government responsibility to solve economic problems:* scale based on perceived government responsibility to solve high taxes (v3717), inflation (v3718), unemployment (v3720), and consumer protection (v3724).

Policy attitudes: Employment: scale based on an item repeated in the pre- and post-election waves, on whether or not the federal government should guarantee everyone a job and a good standard of living (v3241 and v3758). *Health insurance:* preference for a government insurance plan or private insurance (v3273). *Busing:* scale based on the 7-point busing item (v3257), government intervention in school desegregation (v3211), and the number of pro- or anti-busing statements across three responses to the open-ended busing item (v3205–3207); these three items had loaded between .38 and .76 on the second factor of the race factor analysis while all others loaded below .24. *Crime:* scale based on attitudes about the permissiveness of courts (v3913), rights of the accused (v3248), gun control (v3911), the Supreme Court (v3553) and urban unrest (v3767); these had loaded over .30 on a single factor in a factor analysis of eight items on crime policy.

Presidential vote: Post-election report of a Carter (0) or Ford (+1) vote. For the *policy attitude x self-interest interactions,* the policy attitudes were rescored as three-point variables (+1 was closer to Ford, −1 closer to Carter, and 0 equi-distant from them); the interaction terms multiplied this policy-distance measure by self-interest. *Other policy attitudes* were based on a factor analysis of responses to the nine 7-point attitude scales, yielding 3 issue clusters: economic (government medical insurance, government guaranteed job, government aid to minorities, and change in the tax rate); social (rights of the accused, legalization of marijuana, and equal rights for women); and busing (single item). Each issue cluster was recoded into a 3-point dummy variable indicating whether respondents were closer to Ford's or Carter's position in that issue area: +1 if closer to Ford, −1 if closer to Carter, and 0 if equidistant from the two. For analyses involving one of our four policy areas, the relevant issue was not included within its cluster.

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The Levels of Conceptualization: False Measures of Ideological Sophistication

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The "level of conceptualization" index, introduced by Campbell et al. (1960), is commonly used to measure ideological awareness and sophistication among the electorate. Unfortunately, the validity and reliability of the original measure were never sufficiently examined. This article examines the level of conceptualization measures of Field and Anderson (1969) and Nie, Verba and Petrocik (1976). It reaches two major conclusions: (1) the measures under examination are neither reliable nor valid measures of the level of conceptualization construct; and (2) the measures reflect the rhetoric *of contemporary political discourse rather than the actual* process *of political evaluation. These conclusions call into doubt the validity and reliability of the original measure of* The American Voter.

One of the major controversies currently thriving in the field of political behavior concerns the degree of the electorate's ideological awareness. Disagreement centers on how ideological awareness should be conceptualized, on the number of people who think in ideological terms, and on the change in the number of ideologues from the 1950s to the present.

One of the tools used to measure the extent of ideological awareness is the "level of conceptualization" measure, first introduced by Campbell et al. in *The American Voter* (1960, pp. 218–23). "Level of conceptualization" refers to an individual's frame of reference for organizing and evaluating political stimuli. Campbell et al. identified four broad levels, varying in abstraction and generality. At the highest level is an abstract, ideological frame of reference which allows the evaluation of political actors and objects in terms of broad principles. At the lowest level of conceptualization, people evaluate political stimuli in narrow, concrete terms without any issue content. Because level of conceptualization is hypothetically a characteristic of the way in which people think about politics, it is usually assumed to be a fairly stable trait.

Since *The American Voter*, there have been a number of measures of the levels of conceptualization (Broh, 1973; Field and Anderson, 1969; Klingemann, 1973; Miller and Miller, 1976; Nie, Verba and Petrocik, 1976; Pierce, 1970; Pierce, 1975). Unfortunately, the validities of these measures have not been sufficiently examined, and their reliabilities have not been examined at all. Given that much of the controversy over the ideological awareness of the electorate has turned upon questions about the validities and reliabilities of the measures of ideological awareness, an investigation of the measures of the levels of conceptualization is in order.

My purpose here is to examine the validities and reliabilities of two measures of the levels of conceptualization, those of Field and Anderson (1969) and Nie, Verba and Petrocik (1976). The investigation reaches two major conclusions: (1) the measures under examination are neither reliable nor valid measures of the level of conceptualization construct; and (2) the measures reflect the *rhetoric* of contemporary political discourse rather than the actual *process* of political evaluation.

I should make it clear that I am not attacking the level of conceptualization construct, only the measures of it. I assume that the levels exist; however, whether the levels exist or not is logically irrelevant to my thesis that their measures are neither valid nor reliable.

Measures

The original measure of the levels of conceptualization in Campbell et al. (1960) and Converse (1964) was based on a set of eight open-ended questions about the parties and candidates. The respondents were asked what they liked and disliked about each of the two major parties and their presidential candidates.[1]

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[1] The questions were of the form: "I'd like to ask you what you think are the good and bad points about

Each respondent was classified on the basis of a reading of his or her interview protocol (i.e., the responses were not coded as an intermediate step; the verbatim responses, rather than pre-coded responses, were used in classifying the respondents).

The authors of *The American Voter* believed that people would reveal the level of abstraction at which they thought about politics in their answers to the questions regarding their likes and dislikes about the parties and candidates. They assumed that the language in which people responded to the questions would be stable over time. Thus, if a person thought about politics at a certain level, he or she would consistently respond to questions in terms reflecting that level; the response would not reflect, for instance, the rhetoric of a recently viewed political advertisement on television.

The measure classified people into four levels, or frames of reference–(a) ideology, (b) group benefits, (c) nature of the times, and (d) no issue content. Each level hypothetically corresponded to a level of "conceptual sophistication" at which voters functioned when they evaluated the candidates and parties.[2] The authors of *The American Voter* wrote that the highest level, ideological conceptualization, embraced "all respondents whose evaluations of the candidates and the parties have any suggestion of the abstract conception one would associate with ideology" (1960, p. 222). The second level, comprising those who conceptualize politics in terms of group benefits, "was reserved for persons whose issue comment revolved around fairly concrete and short-term group interest, or what we have already described in some detail as 'ideology by proxy' " (1960, p. 223). The third level, nature of the times, contained "persons engrossed in simplistic associations between the 'goodness' and 'badness' of the times and the identity of the party in power, or who appeared to have exhausted their view of the situation with mention of some rather isolated and specific issue" (1960, p. 223). The fourth and lowest level, no issue content, "contains individuals who evaluated the political objects without recourse to issues that might fairly be related to debates over domestic public policy" (1960, p. 223).

The findings presented by Campbell et al., and later elaborated upon by Converse (1964), added a new dimension to the study of public opinion and quickly assumed great importance in the scholarly debate on the ideological awareness of the electorate. Since *The American Voter,* a number of investigators have developed their own measures of the levels of conceptualization. Because coding the individual interview protocols one by one is time-consuming and expensive, most subsequent investigators have relied on the Party and Candidate Master Codes.[3]

Because the later measures of the levels are based on the Master Codes, not the interview protocols, some of the subtlety of the original measure is lost (although exactly how much is not known). While mentions of specific issues can be detected in the Master Codes, the more subtle qualities of "the degree of differentiation and the 'connectedness' of the ideas in which critical issue perceptions were embedded," are not detectable (Campbell et al., 1960, p. 223). The users of these newer measures acknowledge these deficiencies, but argue that their measures are sufficiently like the original to be substituted for it.

The Field-Anderson version of the levels of conceptualization has three levels–explicit ideologue, implicit ideologue and non-ideologue. Describing their measure, Field and Anderson write, "We distinguish between two kinds of ideological statement, one which features explicit use of liberal-conservative terminology and another which stresses several implicitly ideological themes: the individual and the state, the role and power of government, welfare, free enterprise, and posture toward change" (1969, p. 386). Field and Anderson distinguish between the two categories on the basis of language–whether a remark is an explicit use of an ideological term or an implicit reference to an ideological problem or concern (without the use of an ideological term), unlike Campbell and his colleagues–who attempt to distinguish between different levels of sophistication of thought. In addition, Field's and Anderson's measure is more lenient than *The American Voter* measure, since a single remark, rather than a coherent pattern of comments, is sufficient to

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the two parties. Is there anything in particular that you like about the Democratic party?"

[2] Throughout the remainder of this article, the terms "ideological awareness," "level of conceptualization," and "conceptual sophistication" will be used interchangeably.

[3] The exceptions are Klingemann and Wright (Klingemann, 1973) and Miller and Miller (1976), who replicated the levels by the original method of reading the individual interview protocols.

place a respondent in one of the ideological categories.

The Nie, Verba and Petrocik measure is more complicated than the Field-Anderson measure, since it delimits a full range of levels rather than simply distinguishing two types of ideologues from the rest of the population. The measure has seven levels–(a) ideologue, (b) near-ideologue, (c) group benefits and issues, (d) issues, (e) group benefits, (f) party, and (g) apolitical or nature of the times (see Table 1).[4] Although the categories in the Nie et al. measure differ from the categories used in *The American Voter* measure, they were designed to reflect the same hierarchical distinctions in conceptual sophistication.

Both the Field-Anderson and the Nie et al. measures differ from *The American Voter* measure in that they have separate party and candidate components, based on the separate indexing of the responses to the party and candidate questions. The party and candidate components are combined into the overall measures by assigning respondents to the highest levels they achieve on either component.

Reliability

Before examining the properties of these two measures, we must consider how stable we should expect them to be. The authors of *The American Voter* clearly saw the levels as fairly permanent individual characteristics. Changes in presidential campaign styles and changes in individuals' political interest and involvement from one election to the next were expected to move some people from one level to another. However, Campbell et al. expected very little change, most of which they thought would occur between adjacent levels in the upper half of the scale (1960, pp. 255–56).

Field and Anderson interpreted *The American Voter*'s position on the stability of the levels as follows:

> In discussing the general applicability of the 1956 findings, Campbell et al. minimized the prospects for variation, particularly at the level of ideology. Their argument rests largely on the proposition that people conceptualize politics in fairly fixed ways and do not adopt new modes of analysis easily. Essentially the same perspectives will be employed regardless of what kinds of appeals the candidates make. Those unaccustomed to thinking in terms of liberal-conservative distinctions are not likely to

Table 1. ***The Changing American Voter*'s Level of Conceptualization**

Apolitical or nature-of-the-times responses:	These respondents consistently gave nonpolitical responses ("I like Eisenhower," "My husband is a Democrat"), or nature-of-the-times responses. Those unable to answer the question were also coded here.
Party responses:	Any vague reference to party moves a respondent out of the apolitical nature-of-the-times category. A party response might be: "I've always been a Democrat," "The Republicans are the better party," or "Ike's a Republican."
Group benefit responses:	A respondent is moved into this category if he mentions the benefits a particular group will be given or denied, that is, "Democrats do more for the working man like me."
Issue references:	A respondent is placed in this category if one or more of his responses pertain to a particular issue.
Group benefits and issues:	A respondent is moved into this category if his evaluation of the parties or candidates involves references to both group benefits and issues.
Near Ideologues:	Respondents are placed in this category if they offer at least one explicit or implicit ideological mention but fail to give any issue responses or group benefit responses.
Ideologues:	Respondents are put into this category only if they make at least one implicit or explicit ideological mention and also make some reference to issues and group benefits.

Source: Quoted from Norman H. Nie, Sidney Verba and John R. Petrocik (1976). *The Changing American Voter.* Cambridge, Mass.: Harvard University Press, Appendix 2C, p. 375.

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[4] For a complete description of the measure, see Nie et al., 1976, pp. 18–23, and Appendix 2C.

> do so simply because in a given election campaign, stimuli come garbed in ideological terminology (1969, p. 382).

Field and Anderson demonstrated that some changes in conceptualization levels occur in response to changing presidential campaign styles. Specifically, they found a rise in the proportion of ideologues from 1956 to 1964, which they attributed, in part, to the difference between the bland campaign of Dwight Eisenhower and the ideologically charged campaign of Barry Goldwater. They concluded that, at least to some extent, people evaluate political parties and presidential candidates according to the standards of the current campaign, be they ideological or whatever.

Although Field and Anderson demonstrated that the levels varied over time, they maintained that level of conceptualization is a fairly stable attribute.[5] "The Campbell et al. notion of 'cognitive limitations' would seem to apply," they wrote, "but not as rigidly as they surmised on the basis of their 1956 findings" (1969, p. 397). In short, Field and Anderson argued for a weakening of the "conceptual permanence thesis," not its complete rejection.

Nie, Verba and Petrocik expanded upon the work of Field and Anderson, examining the levels from 1952 to 1972. They found a substantial change in the proportion of ideologues. According to their measure (1976, p. 115), the proportion of ideologues varied from 12 percent in 1956 to 33 percent in 1972. But as Campbell et al. had predicted, almost all of the change occurred in the upper levels of the scale. Nie et al. concluded (1976, p. 117) that cognitive limitations "might inhibit citizens from moving out of the lower reaches of the scale. Indeed, the notion of cognitive limitations may more appropriately apply to the lower half of the levels of conceptualization scale than to the upper."[6] The authors of *The Changing American Voter* concluded their discussion of the levels by declaring that while they had shown that the levels vary with the ideological tone of the times, cognitive limitations are still the basis of conceptual sophistication.

In sum, Field and Anderson and Nie et al. did not so much contradict *The American Voter*'s explanation of the sources of the levels as expand on it. *The American Voter* maintained that the levels were primarily determined by cognitive abilities. Field and Anderson and Nie et al. argued that environmental factors form a second set of influences on conceptual sophistication, and that those environmental factors primarily influence people who are more cognitively sophisticated.

Before discussing what level of reliability we should expect, let us consider whether the 1956–1960 test-retest coefficients can be interpreted as reliability coefficients.[7] Is change in the measured levels the product of real change or instrument error? If we find that there was little likelihood of much real change in the levels, then we can treat the test-retest coefficients as reliability coefficients.

Because cognitive abilities are mainly permanent traits, to the extent that they influence the level of conceptualization, there should be no change whatsoever. Beyond this lower threshold, we must ask whether environmental stimuli were likely to have produced much change between 1956 and 1960. The answer is no.

The 1956–1960 period was a time of political calm. The late fifties have often been characterized as a period when few political changes were taking place. A popular theme among academics was the "end of ideology." The nature of the times gives us little reason to expect very much environmentally induced change in the levels (see Converse, 1970, pp. 170–71).

The available evidence about the ideological awareness of the electorate suggests that there was little real change. The marginals for our two measures changed very little from 1956 to 1960 (see Tables 2 and 3). Both measures show small increases in their highest levels. The Nie et al. measure shows only trivial changes in its lower six levels. In short, there is ample

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[5] Since Field and Anderson found that the levels change over time, the notion of stability might seem confusing. What they meant was that individuals who moved did so mostly between adjacent levels and in the direction of the aggregate shift. A situation of instability would exist if individuals were to move across several levels at a time or if a great number of individuals were to move in both directions on the scale. With any given set of marginals, either situation–high or low rates of individual change–could occur.

[6] Nie et al. also noted that Klingemann's and Wright's findings support their position that movement is limited to the upper levels of the scale (p. 117).

[7] The term "reliability" refers to the squared correlation between the "true" score and the observed score (which equals the ratio of true score variance to observed score variance). The term "stability" refers to the correlation of true scores at two points in time.

Table 2. The Field and Anderson Levels of Conceptualization, 1960 by 1956

		1956				
		Non-Ideologue	Implicit Ideologue	Explicit Ideologue	Total	N
	Non-Ideologue	74%	49%	46%	68%	
		(666)	(96)	(57)		819
1960	Implicit Ideologue	13	29	20	16	
		(114)	(57)	(25)		196
	Explicit Ideologue	13	22	34	16	
		(114)	(42)	(43)		199
	Marginals	74%	16%	10%	100%	
	N	894	195	125		1214
		Tau-b = .24				

Source: Data from the 1956–1960 National Election Panel Study, Center for Political Studies, University of Michigan.

Note: The marginal distributions here do not match those given in Field and Anderson because their data were from the 1956 and 1960 cross-sectional samples, while these data are from the smaller panel sample.

evidence for our interpretation of test-retest coefficients as reliability coefficients.

Because level of conceptualization rests on a fairly permanent foundation of cognitive structure and party identification does not, we should expect greater variation over time in the latter. In other words, events should produce more change in party identification than in level of conceptualization. Therefore, we should expect the level of conceptualization measures to have test-retest reliabilities at least as high as that of party identification.

Table 3. The Nie, Verba and Petrocik Levels of Conceptualization, 1960 by 1956

		1956								
		Apolitical	Party	Groups	Issues	Groups/ Issues	Near-Ideologues	Ideologues	Total	N
	Apolitical	42%	11%	7%	8%	2%	5%	1%	8%	91
		(32)	(18)	(12)	(15)	(3)	(8)	(3)		
	Party	13	24	10	13	4	8	3	10	120
		(10)	(39)	(17)	(26)	(7)	(13)	(8)		
	Groups	20	13	22	15	10	14	9	14	166
		(15)	(22)	(37)	(30)	(15)	(22)	(25)		
1960	Issues	16	13	11	22	16	12	12	14	175
		(12)	(22)	(18)	(44)	(25)	(20)	(34)		
	Groups/Issues	5	8	14	10	25	10	9	12	142
		(4)	(13)	(24)	(19)	(39)	(16)	(27)		
	Near-Ideologue	4	10	12	10	10	17	15	12	144
		(3)	(16)	(20)	(19)	(16)	(28)	(42)		
	Ideologue	1	21	24	24	33	34	51	31	376
		(1)	(35)	(41)	(47)	(51)	(54)	(147)		
	Marginals	6%	14%	14%	16%	13%	13%	24%	100%	
	N =	77	165	169	200	156	161	286		1214
		Tau-b = .30								

Source: Data from the 1956–1960 National Election Panel Study, Center for Political Studies, University of Michigan.

The Data

The data are from the Center for Political Studies' 1956–1960 National Election Panel Study. The panel respondents form a subset of the 1956 and 1960 cross-sectional samples consisting of those who were interviewed in both years. The panel allows the examination of individual change, as well as aggregate change, from 1956 to 1960. With the data on individual change, we can assess the reliabilities of the levels measures.

Let us now consider the evidence. Table 2 shows a cross-tabulation of the Field-Anderson measure across the 1956–1960 period. The test-retest coefficient, Kendall's tau-beta, is 0.24, indicating a remarkably low test-retest reliability. Not only is this level of reliability far lower than that of party identification, but also it is lower than all but two of the eight issue-items which Converse attacked in "Attitudes and Nonattitudes" (1970).[8]

Table 3 shows a cross-tabulation of respondents' scores on the Nie et al. measure over the 1956–1960 period. Here, the reliability is somewhat higher, 0.30, but here, too, the reliability is nowhere near what we expect. The reliability coefficient is less than half the size of the party identification reliability coefficient.[9]

Of course, we must not rely on the coefficients alone. These two tables tell far more about the reliabilities of the measures than can be expressed in two coefficients. Let us consider what we should expect to see in these tables.

The investigators who developed these measures expected most of the movement to be among adjacent levels in the upper half of the scale. Very few people were expected to move more than one level from election to election or to move out of the lower levels of the scale. Noting that neither Field and Anderson nor Pierce (1970) found any aggregate changes in level of conceptualization in the lower 40 percent of the population from 1956 to 1964, Converse declared,

> This constancy at lower levels is in many senses more surprising than the gains at higher levels, given what we know of the nature of political stimulation at the time. It suggests a resistance to change which is quite remarkable (1975, p. 100).

Field and Anderson and Pierce found that from 1956 to 1964, there was no change in the proportion of the population falling in the lower levels of their scales. They inferred that there was little or no individual change. They were wrong. Of the 34 percent of the population who were in the lower three levels of *The Changing American Voter* scale in 1956, less than half remained in one of those levels by 1960. Fifty-one percent of those in the lower levels moved out of them in a four-year period. This is hardly a "remarkable" resistance to change.

The reasoning of Converse and the others we have discussed is based on an ecological fallacy. From aggregate data showing no change in the distribution of the lower levels, they concluded that there was little or no individual change. Yet the data show that their inference of high stability in the lower portion of the scale was totally wrong.[10]

Table 4 summarizes the turnover in *The Changing American Voter* measure. As can be seen, there was a high rate of change from 1956 to 1960; 46 percent moved two or more levels. Some respondents actually moved between the lowest level and the two ideological levels at the top of the scale.

The party and candidate components of our two measures (shown in Tables 5 and 6) help us examine the levels in more detail. The most striking observation that emerges from an examination of these tables is that the party components of each measure are far more reliable than the candidate components. The reliability coefficient for the party component of the Field-Anderson measure is 0.31, while the candidate component is zero. There is actually no relationship between the respon-

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[8] Upon discovering such distressingly low test-retest correlations among attitude items, Converse concluded, "We were doing a very poor job of tapping the attitudinal dimensions at which we originally aimed, . . . our results . . . give witness to an incredible degree of measurement unreliability" (1970, p. 171).

[9] An examination of Table 3 suggested that trichotomizing the Nie et al. measure might increase its reliability. Two alternative trichotomies were tested. Neither version produced a coefficient significantly different from that of the uncollapsed measure.

[10] Another example of reasoning based on the ecological fallacy is from Pierce, who writes, "The increase in the ideologues seems to have come at the benefit of the 'group interest' categorization. That is, the decrease in the percentage on the group interest level is very similar to the increase in the ideologue level. So, it appears that many individuals merely moved up from one level to the next in their conceptualization of political phenomena" (1969, pp. 34–35).

Table 4. 1956–1960 Turnover in *The Changing American Voter* Levels of Conceptualization

Number of Levels Moved from 1956 to 1960								
0	1	2	3	4	5	6	Total	N
30%	24%	19%	14%	8%	4%	*	100%	
(366)	(287)	(231)	(170)	(102)	(54)	(4)		1214

Source: Data from the 1956–1960 National Election Panel Study, Center for Political Studies, University of Michigan.

*Less than ½ percent.

dents’ scores on the candidate scales for 1956 and 1960. *The Changing American Voter* measure fares somewhat better, but not much so. The party component has a reliability of 0.35, while the candidate component has a reliability of 0.20. What reliability these measures have is mostly due to their party components.

The finding that the party components are more reliable than the candidate components is consistent with previous analyses which have shown more aggregate change in the candidate components than in the party components. Upon examining the separate components of their measure, Field and Anderson concluded, “The parties are consistently the main bearers of ideology, regardless of who is running for president” (1969, p. 388). In addition, they found that the increase in the combined scale from 1960 to 1964 was largely due to the “threefold increase of Ideologues in the candidate reference” (1969, p. 388). Nie and his colleagues concurred, declaring, “It is the

Table 5. The Field and Anderson Party and Candidate Components, 1960 by 1956

		A. Party Components				
		1956				
		Non-Ideologue	Implicit Ideologue	Explicit Ideologue	Total	N
	Non-Ideologue	79%	48%	40%	71%	
		(742)	(92)	(33)		867
1960	Implicit Ideologue	14	34	19	17	
		(128)	(65)	(16)		209
	Explicit Ideologue	7	19	41	11	
		(68)	(36)	(34)		138
	Marginals	77%	16%	7%	100%	
	N	938	193	83		1214

		B. Candidate Components				
		1956				
		Non-Ideologue	Implicit Ideologue	Explicit Ideologue	Total	N
	Non-Ideologue	94%	100%	92%	94%	
		(1080)	(15)	(46)		1141
1960	Implicit Ideologue	*	0	2	1	
		(6)	(0)	(1)		7
	Explicit Ideologue	6	0	6	5	
		(63)	(0)	(3)		66
	Marginals	95%	1%	4%	100%	
	N	1149	15	50		1214

Source: Data from the 1956–1960 National Election Panel Study, Center for Political Studies, University of Michigan.

*Less than ½ percent.

Table 6. *The Changing American Voter* Party and Candidate Components, 1960 by 1956

		A. Party Components								
		1956								
		Apolitical	Party	Groups	Issues	Groups/ Issues	Near-Ideologues	Ideologues	Total	N
	Apolitical	47% (104)	25% (47)	14% (36)	17% (23)	9% (17)	20% (8)	4% (8)	20%	243
	Party	14 (30)	22 (41)	9 (23)	15 (20)	8 (16)	5 (2)	4 (7)	11	139
	Groups	14 (32)	13 (25)	33 (83)	10 (13)	17 (32)	10 (4)	14 (24)	18	213
1960	Issues	11 (25)	18 (34)	10 (25)	28 (39)	14 (27)	12 (5)	6 (10)	14	165
	Groups/Issues	6 (13)	7 (14)	16 (40)	10 (14)	25 (48)	2 (1)	10 (17)	12	147
	Near-Ideologues	5 (11)	7 (13)	2 (6)	7 (9)	3 (6)	25 (10)	13 (23)	6	78
	Ideologues	4 (8)	9 (17)	16 (41)	14 (19)	24 (46)	25 (10)	50 (88)	19	229
	Marginals	18%	16%	21%	11%	16%	3%	15%	100%	
	N	223	191	254	137	192	40	177		1214
		Tau-b = .35								

		B. Candidate Components								
		1956								
		Apolitical	Party	Groups	Issues	Groups/ Issues	Near-Ideologues	Ideologues	Total	N
	Apolitical	42% (67)	12% (43)	12% (13)	10% (21)	9% (6)	7% (11)	5% (7)	14%	168
	Party	21 (33)	28 (98)	27 (30)	20 (44)	12 (8)	19 (30)	22 (32)	23	275
	Groups	11 (18)	12 (42)	13 (15)	18 (40)	8 (5)	10 (16)	12 (18)	13	154
1960	Issues	8 (13)	14 (49)	13 (15)	15 (33)	18 (12)	18 (29)	17 (25)	14	176
	Groups/Issues	4 (6)	6 (21)	8 (9)	9 (20)	17 (11)	9 (15)	7 (10)	8	92
	Near-Ideologues	6 (10)	12 (41)	9 (10)	9 (20)	11 (7)	16 (26)	13 (20)	11	134
	Ideologues	7 (11)	16 (55)	18 (20)	19 (42)	26 (17)	21 (33)	25 (37)	18	149
	Marginals	13%	29%	9%	18%	5%	13%	12%	100%	
	N	158	349	112	220	66	160	149		1214
		Tau-b = .20								

Source: Data from the 1956–1960 National Election Panel Study, Center for Political Studies, University of Michigan.

changing candidates rather than the parties that provide the major change in political content" (1976, p. 113).

Given that the major change in the political environment from 1956 to 1960 was the change in presidential candidates, we should expect greater change in the candidate components than in the party components. However, the candidate component reliabilities of zero and 0.20 are shockingly low. If we were to accept the position that the measures are valid indicators of conceptual sophistication, then we would have to conclude that the conceptual level at which voters evaluate candidates is almost entirely environmentally determined, and that cognitive limitations barely influence candidate evaluations. In other words, the measures show that voters do not maintain any set standards by which they evaluate candidates, that voters' standards vary from election to election. Of course, the alternative interpretation of these data (which seems far more reasonable) is that the measures are not valid.

Although the party components change less than the candidate components, their reliability coefficients of 0.31 and 0.35 are far lower than they should be. There is a high rate of change, despite the fact that political parties are relatively unchanging parts of the environment.

Although the low over-time correlations of the measures is strong evidence that the measures are not reliable, the possibility has not been logically eliminated that massive hidden change over the 1956–1960 period only makes the measures seem unreliable. Fortunately, in the case of one of the measures, *The Changing American Voter* party component, we can formally test the reliability versus stability arguments. We can do so by using the estimators of reliability and stability developed by Wiley and Wiley (1970).

The Wiley and Wiley method for separating reliability and stability requires measurements for each respondent at three or more times. The 1956–1960 SRC panel study has a 1958 wave in which respondents were asked what they liked and disliked about each of the two major parties; thus, we have the necessary three replications of the party component.[11] *The Changing American Voter* party component is the best one to test, since its over-time correlation is the highest of all the measures. That is, we will test the best of all the measures. If anything, the others are even less reliable.

The results confirm the above conclusions. The reliability coefficient of the measure was 0.33 in 1956, and 0.50 in 1958 and 1960. The stability coefficients over the 1956–58 and 1958–60 periods are an impressive 1.00. In other words, virtually *all* of the change in the observed measures over the 1956–1960 period is due to measurement error. The underlying trait is nearly constant.

There is one more way to assess these measures. Our picture of unreliability might be mitigated somewhat if a portion of the change were to be explained by the theory from which the level of conceptualization measure is drawn. I noted above that at least some change was to be expected. Specifically, *The American Voter* suggests two likely causes of change over time—changes in political interest and changes in political activity (1960, pp. 251–55). Using their discussion, we can hypothesize that changes in level of conceptualization will vary directly with changes in (a) interest in politics, (b) attention to politics in the media, and (c) political activity. We would expect anyone who becomes more interested in politics, pays attention to politics in an increased number of media, or becomes more politically active, would develop a higher level of conceptualization.

None of these hypotheses is supported by the data (not shown).[12] Correlating each of the three hypothesized sources of change with change in the overall *Changing American Voter* measure and its two components yields nine possible relationships. Of the nine, only one is statistically significant; none is substantively significant. The only one that attains statistical significance at the .05 level is the relationship between change in media attention and change in the overall measure, which has a tau-beta of only .05. Thus, we can conclude that change in level of conceptualization is not caused by change in political interest, attention to politics in the media, or political activity. This finding strongly supports the interpretation that the low test-retest correlations indicate low reliability rather than real change in the distribution of conceptual sophistication, as argued in *The Changing American Voter.* In addition, these data constitute a test of the construct validity of the measures. If there were any real changes in ideological awareness, those changes should be related to parallel changes in interest, attention and activity. The finding that the theoretically predicted relationships do not exist indicates that the measures are not valid.

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[11] Since there was no 1958 presidential election, we cannot use the Wiley and Wiley methods with the candidate components or overall measures. The Field-Anderson measure is not appropriate because it does not approximate an interval level measure, as does *The Changing American Voter* measure.

[12] Change in interest is the difference between the 1960 and 1956 responses to the questions: "Some people don't pay much attention to the political campaigns. How about you? Would you say that you have been very much interested, somewhat interested, or not much interested in following the political campaigns so far this year?" and "Some people don't pay much attention to the political campaigns. How about you? Would you say that you have been very interested, fairly interested, slightly interested, or not at all interested?" The scores for each response were adjusted to make the codings comparable. Change in media attention is the difference between the number of media in which the respondent had seen, heard, or read political items. Change in political activity is the change in the number of activities in which the respondent participated. The possibilities were: (a) attend meetings or rallies, (b) belong to a political club, (c) donate money, (d) work for a party or candidate, (e) wear a campaign button or have a bumper sticker supporting a candidate.

Discussion

We have found that the two measures of the levels are quite unreliable. According to the authors of *The American Voter,* conceptual sophistication is a relatively permanent characteristic. While Field and Anderson and Nie and his colleagues broadened the construct to include a degree of change in response to environmental stimuli, the theoretical expectation of long-term stability based on cognitive limitations remained unchanged. Given that conceptual sophistication is a stable trait, the present findings of unreliability suggest that instead of reflecting conceptual sophistication, the measures are reflecting short-term forces unrelated to conceptual sophistication. In other words, the findings imply that the measures are not valid.

In saying that these scales measure some short-term phenomena rather than the long-term phenomenon, conceptual sophistication, I do not wish to suggest that they are not influenced by it. They may well be. But the test-retest correlations of .24 and .30 indicate that other factors are more influential. The question is, what do these scales measure?

The evidence suggests that there is a disjuncture between the processes by which people evaluate the parties and candidates and the rhetoric in which they discuss their likes and dislikes. In other words, the measures of the levels of conceptualization seem to be tapping the language that the evaluations take after they have been formed, rather than the actual processes of evaluation. Let us consider why.

We may assume that most reports of evaluations occur after the evaluations have been made. In the case of political parties, reports may be given years or even decades after evaluations are first made. In the case of candidates, reports may be given from a few days to many years after evaluations are first made.[13] Therefore, when people are asked what they like about a party or whether anything might make them want to vote for a candidate, their responses will come from brief searches of their memories, rather than directly from their evaluations.[14]

When respondents try to recall what they like about a party or candidate, they will certainly be guided by their evaluations of the party or candidate, but we cannot expect their evaluations to determine the exact language of their responses. A response may include remarks which are characteristic of the respondent's level of conceptualization, but it will probably also include many things which merely reflect the respondent's experiences over the last few days. Consequently, we should expect to hear about things such as reports read in the morning newspaper or seen on the evening television news broadcast or things heard or said in a recent conversation about politics.

The product of a brief memory search over a memory filled with recent events is not likely to be a sound basis for a reliable measure. Although evaluations may remain highly stable, the language of evaluations—examples of the good and bad aspects of the parties and the candidates—are likely to fluctuate considerably. Findings of unreliability should not surprise us.

In the literature on the levels of conceptualization, there is a pattern of anomalous findings which is consistent with the propositions that the rhetoric of evaluation does not mirror the processes of evaluation, and that the measures of level of conceptualization actually measure only the rhetoric of evaluation.

The first evidence is from Field and Anderson. When they built their measure, they distinguished between those who made explicit and implicit ideological remarks, rather than between ideologues and non-ideologues. Field's and Anderson's distinction led them to an interesting finding:

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[13] Although evaluations are, no doubt, often updated as circumstances change and new information becomes available, we can safely assume that most people already have in mind some rough evaluation of the party or candidate when they are asked. This evaluation is stored in the memory.

[14] The theory that responses to attitude items are products of memory searches has been developed in the psychometric literature. See, for instance, Hunter and Gilmore (1974).

> While the proportion of Implicit Ideologues remains quite stable from one election to the next, the number of people volunteering the actual language of ideology is subject to considerable fluctuation. This suggests that terminology is more easily adopted than is concern for substantive ideological problems (1969, p. 388).

In other words, Field and Anderson found that there was no increase in the number of those who evaluated the parties and candidates in terms of ideological concerns, only an increase in the number of those who used ideological rhetoric. In light of the fact that the data covers the period from the bland Stevenson-Eisenhower campaign of 1956 to the fiery Johnson-Goldwater campaign of 1964, we can conclude that there was no increase in the ideological sophistication of the electorate, only an increase in the sophistication of its language.[15]

Much of the ground covered by Field and Anderson was also covered by Pierce (1970). Pierce examined the changes in the proportion of ideologues over the 1956–1964 period using three measures of ideological awareness. Two of his measures are of interest to us, his conceptual and informational measures (1970, pp. 28–30). The conceptual measure was a replication of the levels of conceptualization measure. The informational measure was a measure of "informational ideologues," those who could correctly identify the more conservative of the two major parties and give a "broad, accurate definition of conservatism" (1970, p. 29).[16]

Pierce discovered that his two measures of ideological awareness did not rise and fall simultaneously:

> Although a significant increase in the number of conceptual ideologues was evidenced in 1964, no such increase in the proportion of informational ideologues is found.... One might ask the reason for this and the explanation is probably found in the divergent character of the phenomena examined by the two measures. That is, it would take much less psychological effort for an individual to merely reflect campaign rhetoric emphasizing the ideological character of political agents (the conceptual measure) than for him to increase the mass of information he holds (1970, p. 34).

Pierce's finding that the use of ideological rhetoric increased while the store of ideological knowledge in the population remained constant should not surprise us. It is consistent with Field's and Anderson's finding about the increase of Explicit Ideologues without an accompanying increase in Implicit Ideologues. Both imply that there was a change in the sophistication of the language in which people talked about politics, but no change in the sophistication of the underlying evaluative processes. More generally, both suggest that the factors that influence evaluative rhetoric are not the same as the factors which influence the actual evaluations.

Concluding Comments

The hypothesis that the rhetoric of evaluation does not reflect the process of evaluation could be more precise. People could be responding to the like-dislike items in the rhetoric most commonly used in the current presidential campaign or in the rhetoric used in recent television news programs. The question is, how "short" are the short-term forces that influence the language that people use to evaluate the parties and candidates?

The available evidence suggests that the scales are probably not stable over the period of a single election, that a person's rhetoric is likely to vary in level of conceptual sophistication over the course of a single campaign. As noted above, the more interested in, attentive to, and active in politics one is, the more likely one will be to use ideological rhetoric in evaluations. It follows that if people were to evaluate candidates in language reflecting the ideological tone of *entire* campaigns, then changes from one election to the next in interest, attention, or activity would cause corresponding changes in ideological level of the rhetoric they use. Therefore, the finding that changes in interest, attention and activity did not produce changes in level of conceptualization implies that people evaluate candidates in rhetoric which does not reflect entire campaigns, but shorter-term influences.

Unfortunately, this evidence is not conclusive. In order to assess what the scales actually measure, we must have more than one measurement during a single campaign. Without that information, we cannot be sure whether we are measuring how people respond to the rhetoric of entire campaigns or how they respond to day-to-day changes.

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[15] An alternative way to interpret these data is to treat implicit and explicit ideological remarks as separate measures of sophistication. The failure of the two measures to covary indicates a lack of convergent validity.

[16] Pierce's informational ideologue measure is a replication of Converse's (1964) "strata of recognition and understanding" (see pp. 219–24).

Although this article examines only the Field and Anderson and Nie et al. measures of the levels, one cannot help speculating about the original measure of *The American Voter.* As noted above, the original measure differs from the others in that it was based directly on the interview protocols rather than on the Master Codes. Consequently, we cannot draw any firm conclusions about the original measure. To do so, we must have at least two replications within a single panel study. However, the original measure must now be considered suspect. Given the failure of the two measures examined here, we must assume that the original measure is similarly flawed.

The question to which we eventually come is whether the basic assumption underlying the measurement of the levels is reasonable. Can we really expect people to reveal their levels of conceptual sophistication in response to a set of eight items which do not directly elicit ideological responses? If the Field and Anderson and Nie et al. measures are any indication, the answer is no. In any event, the burden of proof as to the validity and reliability of the original measures now rests on the shoulders of those who would defend it.

One last point remains to be made. As I noted in the introduction, the levels measures have been used to assess the extent of the ideological awareness of the electorate. These measures have been used as evidence that the electorate is becoming increasingly ideologically aware. The evidence presented here does not support that thesis. These measures show only that the electorate has been using ideological rhetoric more often. They do not and cannot show that the electorate is becoming more ideologically aware.

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The Effects of Charges of Corruption on Voting Behavior in Congressional Elections

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This study assesses the electoral impact of charges of corruption on candidates in contests for the U.S. House of Representatives in five elections from 1968 to 1978. This assessment includes a consideration of the victory or defeat of alleged or convicted corrupt candidates, and an examination of the impact of corruption charges on electoral turnout and percentage of votes polled by the accused candidates. While most candidates accused of corruption are reelected, overall they appear to suffer a loss of 6–11 percent from their expected vote. The type of corruption charge is an important determinant of vote loss. Allegations of corruption appear to have little effect on the net turnout.

Even though much has been written about corruption in U.S. elections (for a recent sampling, see Benson, 1978; Gardiner, 1970; Berg et al., 1976; Peters and Welch, 1978; Rundquist et al., 1977), and many laws have been passed in an attempt to reform corrupt campaign practices, we simply do not know very much about the role of corruption charges in influencing voters. Do candidates accused of corruption lose substantial numbers of votes? We also know little of the incidence or distribution of corruption charges among different types of elections or candidates. We cannot state whether candidates charged with, indicted for, or convicted of corrupt acts are electorally punished to any degree, or whether some kinds of unethical behavior, proven or alleged, are judged more harshly by the electorate than others.

This study assesses the electoral impact of charges of corruption on accused political candidates. Examining all the known instances where charges of political corruption or unethical behavior were leveled at candidates for the U.S. House of Representatives in elections from 1968 to 1978, we isolate and determine the degree to which specific corruption allegations diminish the electoral success of accused candidates. This assessment includes a consideration of the victory or defeat of allegedly corrupt candidates, and an examination of the impact of corruption charges on electoral turnout and percentage of votes polled by the accused candidates.

Prior Research

Probably most citizens, if asked, would state that corrupt politicians should not be reelected. Students of democratic theory have argued the need for informed citizens to keep public officials honest through the mechanism of the ballot box (Berelson, 1952; Berelson et al., 1954). The basic assumption underlying this citizen role is that if voters knew a candidate for political office were corrupt (dishonest, unethical) they would not vote for that candidate; and if the candidate were an incumbent, they would "throw the rascal out." Yet this belief that corrupt politicians will be subject to "electoral retribution" contrasts with the striking examples of members of Congress and others reelected by overwhelming majorities even after indictments or convictions for gross violations of the public trust. What factors explain this apparent discrepancy? Why might corrupt politicians find favor with voters? The works of Rundquist et al. (1977) and Rundquist and Hansen (1976) examine the reasons why voters would support corrupt politicians.

One commonly accepted explanation is that voters are uninformed or misinformed about candidates. A voter may in fact honestly be ignorant of a candidate's actual or alleged corruption (Bryce, 1959; Levin, 1960). Even when adequate and reliable information about the proven corrupt activities of a public office seeker is available, a segment of the voting public is likely to misperceive, selectively perceive, or, under some circumstances, refuse to believe the truth of the charges, depending on the credibility of the information source (Gardiner, 1970). Still, there are instances when voters support a candidate, knowing he or she has been convicted of a crime. Lack of knowl-

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edge, therefore, cannot explain all instances of voting for a corrupt candidate.

A second explanation suggests that voters may knowingly vote for a corrupt politician by trading their vote for material advantage, particularly economic reward. Therefore, some political analysts have argued that electoral constraint of corrupt politicians cannot be accomplished because of the "venality" of voters, themselves capable of being corrupted. The "venal voter" has often been identified as a member of one of the ethnic groups associated with the rise of urban political machines (Wilson, 1966; see also Gosnell, 1967; McCook, 1892; Speed, 1905; Banfield and Wilson, 1963).[1]

Reformers and "good government" advocates have stressed citizen education programs as a means of eliminating corruption in government. George Benson (1978) takes this position when he argues for the education of citizens, particularly the "newer ethnic" groups, into the democratic way. Riker (1965, pp. 64–65) maintains that one important reason for our level of corruption appears to be "that not all Americans have been taught to value democracy," but argues that issue-oriented elections will diminish voting for corrupt candidates (cf. Bryce, 1959; Brogan, 1933).

A third reason why citizens vote for corrupt politicians is that voters see corruption as "functional" to society. While those theorists just discussed see corruption as a "disease" or a "pathology" (see also Berg et al., 1976; Rogow and Lasswell, 1966), others see it as a necessary evil. Citizens in some political cultures, including some subcultures in the United States (cf. Elazar, 1972), see corruption as a fact of life, a normal practice in carrying out public (and perhaps private) business. Corruption becomes the necessary "grease in the wheels" of governmental policy making (Wilson, 1966; Ford, 1904). As one journalist recently put it:

> Normal, everyday greed aside, corruption is often a sign something is amiss with our laws and institutions. It is often cheaper and quicker to buy a bureaucrat than to explain something to him (Von Hoffman, 1978, Sec. 1, p. 10).

Still others view corruption as "functional" to the maintenance of a political system when the official agencies of government are not fulfilling a specific demand of the people or providing a needed service. This approach, of course, is the one taken by Merton (1968) and Wolfinger (1972) in explaining the rise of bossism in the U.S.[2]

A fourth explanation for the success of some corrupt or allegedly corrupt candidates is that citizens weigh corruption charges in their voting decision along with other issues, party identification and other factors (Rundquist et al., 1977). It is not accurate to assume that every voter uses it as the sole criterion for electoral choice, regardless of partisanship or other issue considerations. If the voter feels strongly about honesty in government, the belief that one candidate is corrupt may be enough to crystallize that voter's decision to cast a ballot against that candidate. If, on the other hand, a voter likes a particular candidate's political party or stand on important issues, the voter may discount any corruption charge leveled against the candidate and vote for him (or her) anyway. This discounting is particularly easy to do when the charges of corruption have not been proven. Furthermore, where voters are completely indifferent to charges of corruption, they would ignore them when it is time for a vote decision. Using results generated from "experimental elections," Rundquist et al.

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[1] Many readers will recognize, of course, that this is the logic underlying Banfield's and Wilson's (1963) categorization of individuals into "private-regarding" (those who view politics as a way of getting ahead and therefore more corruptible) and "public-regarding" (those who view politics as the pursuit of the public interest).

[2] In an attempt to reconcile the differences between an approach which suggests that corruption is a "pathology" and one which claims it is "functional," Walter Lippman, in explaining the corrupt Walker-Tammany regime in New York City, claimed they were natural, older, forms of political association. He argued that those political associations

> which bind together masses of people in a complex of favors and coercions, are the ancient form of human association. They might be called natural governments. Our modern, artificial constitutions were super-imposed upon them partly by coalitions of the stronger factions and partly by compromises of interest. The natural governments are not abolished when this happens. They continue to a decisive degree to operate through the artificial government. . . . My point is that Tammany is not a disease but simply the old body politic in its more or less natural state, and that the American ideal of government as a public trust to be carried on by disinterested men represents not the actuality but a long step ahead in the evolution of man (quoted in Heidenheimer, 1970, p. 295).

Lippman's argument is very similar to the thinking of Huntington (1968) and Scott (1972) who relate official corruption in political regimes to levels of economic development.

(1977) found support for their reasoning that voters "trade" the fact that a candidate is corrupt for other desirable benefits, such as having what voters see as the correct stance on a policy issue. These conclusions were arrived at by observing the relative proportions of voters who would and would not switch their votes to a less-preferred alternative when they learned that solid evidence pointed to their first choice as corrupt.

We are strongly persuaded by the "trading" argument because it accounts for the success of so many allegedly corrupt candidates in terms of a broader theory of electoral behavior. It does so by taking into account the great variations in citizen attitudes toward corruption in general and toward particular acts of corruption. However, Rundquist and his associates used data only from experimental settings. They did not attempt to see what net effect corruption charges actually had on a candidate's vote totals in real elections. In other words, as a short-term force, how potent are charges of corruption in influencing actual vote totals and electoral turnout? Our aim, therefore, is to extend the logic of the implicit trading theory by examining the effect of allegations of corruption on voting in natural settings, namely the congressional elections between the years 1968 and 1978. Thus we shift the emphasis from the question of why voters support corrupt or allegedly corrupt politicians to an examination of the extent to which the electorate punishes the candidate accused or convicted of corruption.

Electoral Retribution

The punishment levied by voters on corrupt or allegedly corrupt candidates we will call "electoral retribution" (ER). The democratic system, with its regular electoral check on political officials, forces politicians to ask the question: how will this decision, or this action, or that behavior, affect my chances in the next election? Some have argued that this is the best sanction devised to control corruption among public officials (Riker, 1965, p. 297). In an election where one candidate is accused of corruption, the voter is confronted with a range of alternatives, some of which could punish the allegedly corrupt candidate. These alternative conditions are listed below, assuming only contests with two candidates:

1. Voter originally in favor of C (allegedly or convicted corrupt candidate) switches to NC (non-corrupt) (ER).
2. Voter originally in favor of C decides to abstain (ER).
3. Voter originally in favor of C stays with C.
4. Voter originally abstaining switches to NC (ER).
5. Voter originally abstaining switches to C.
6. Voter originally abstaining continues abstention.
7. Voter originally in favor of NC remains with NC.
8. Voter originally in favor of NC switches to C.
9. Voter originally in favor of NC abstains.

Without attempting to establish exact probabilities of each of these events occurring, let us consider each one. Electoral retribution (ER) would occur when conditions 1, 2, or 4 are met. The clearest form of retribution occurs when the voter casts a ballot for the non-corrupt candidate, even though the alleged or convicted corrupt candidate was originally the preferred alternative (condition 1). The voter could also decide not to vote in that particular contest (condition 2). Such an occurrence is likely to happen when the alleged or convicted corrupt candidate is the preferred alternative and the non-corrupt candidate is an unacceptable second choice. Corruption charges can also bring about retribution by activating voters who were indifferent to both candidates to go to the polls and cast a ballot for the non-corrupt candidate (condition 4).

Since we know there are voters distressed over political corruption, we assume that conditions 1, 2 and 4 occur with some unknown, but probably considerable, regularity in elections where one candidate is accused of corrupt or unethical behavior. However, the evidence of Rundquist et al. (1977) also shows that other voters find that party stance, issue position, or personality outweigh the impact of alleged corruption; these voters would be expected to retain their original choice (conditions 3, 6 and 7). It seems highly unlikely that charges of corruption against a candidate would cause voters to defect *to* the allegedly corrupt candidate from a position of abstention or support for the non-corrupt candidate. Thus, we can assume that conditions 5 and 8 occur infrequently. Condition 9 would also seem to be rare, though for some voters a charge of corruption against a candidate may increase or produce feelings of alienation from politics, causing them to abstain.

Thus, our measure of electoral retribution is simply the excess of voters in categories 1, 2 and 4 (those who switch from the allegedly corrupt candidate to the non-corrupt candidate or leave a position of abstention to vote against the allegedly corrupt candidate)—over the voters in categories 5, 8 and 9 (which for all practical purposes is probably very close to zero).

Changes in turnout also are potentially affected by corruption charges. However, the aggregate impact on turnout may not be so clearly discernible, since conditions 2 and 9 would tend to decrease turnout, conditions 4 and 5 to increase it. Again, since conditions 5 and 9 seem fairly unlikely, the shifts in turnout rest on the relative and contradictory effects of voters for C who decide to abstain (condition 2) and potential abstainees who decide to vote for NC (condition 4).

We do not expect that electoral retribution or changes in turnout will be constant for all elections. We hypothesize that there are certain independent factors which have an impact on the extent of electoral retribution. Those which we will examine include congressional seniority, the partisan affiliation of the candidate, and the nature and severity of the corruption charge.

As a general rule, we expect to find that the longer the tenure or seniority of the candidate charged with corruption, the more immune that official will be either to defeat or to a decrease in their expected margin of victory. The reason, of course, is that the incumbent has had time in office to establish a record by providing services and taking positions which were popular in the home constituency. It becomes more difficult, then, for a voter to trade away those desired benefits than if the accused is a relatively new member of Congress or a challenger who has not yet served.

Given the nature of the composition of the Democratic electorate, we expect to find that Democrats accused of corruption suffer more electoral retribution in the form of defeats and smaller winning margins than Republican candidates accused of corruption. Since voting behavior studies find the average Democratic party member to be less loyal, with a weaker sense of partisan identification, than the average Republican, it seems plausible to expect that corruption charges will have a greater impact on the vote decisions of Democrats than Republicans (Campbell et al., 1960). In other words, all short-term forces, including allegations of corruption, may be more important to Democrats than Republicans.

Finally, we know that all charges of corruption are not the same; some are judged more harshly by citizens and members of the elite than others (Peters and Welch, 1978). Thus, we hypothesize that the degree of electoral retribution suffered by a candidate is directly related to the severity of the corruption charge. For example, we expect that charges of bribery will have more of an impact on electoral defeat and retribution than campaign fund violations or misuses of congressional prerogatives.

Thus, to summarize, we will test the following hypotheses:

HYPOTHESIS 1. *Candidates accused of corruption will suffer a net loss of votes compared to the expected party votes in the district. This loss will be greatest when the candidate possesses the following characteristics:*

A. Is a Democrat,

B. Is a nonincumbent or has little seniority,

C. Commits a more rather than less severe act of corruption (see discussion in next section for further operationalization of this term).

HYPOTHESIS 2. *Aggregate turnout in an election where one candidate is accused of corruption will not be affected because of the contradictory effects of the corruption charge (conditions 4 and 5 "washing out" conditions 2 and 9).*

We would expect, however, that studies based on individual-level survey data would show a change in the composition of the turnout.

Study Design and Methods

The Data Base. In order to test completely the dimensions of electoral retribution, the researcher would have to do longitudinal analysis on individual voters in a number of districts where one candidate is accused of corruption. Clearly, such a data collection enterprise is not feasible without substantial resources. We can, however, in a more limited sense but still with satisfying results, assess the magnitude of electoral retribution by analyzing all the instances where corruption charges played a prominent role in election campaigns. This we did for the six congressional elections between the years 1968 through 1978. All accused candidates were male. For each we recorded such factors as his political party, whether or not he was an incumbent, the number of congressional terms he had served, the severity of the charge, the type of charge, the turnout in the current and past two elections, the vote he or his party had polled in each of the past two elections and the vote in the election where corruption was alleged. Additionally, several demographic characteristics for the district of each accused candidate were collected (Barone et al., 1978), such as its location, the median voting age in the district, the percent of the district composed of inhabitants of the central city, the percent composed of residents of foreign stock, and residents' median income. As a control, similar information on a random sample of congressional elections not involving corruption charges was recorded for each election year. In total, our data base contained 83 districts where there were corruption charges and 193 control districts.

The type of charge was coded into a sevenfold classification: bribery, conflict of interest, campaign violations, morals charges, abuse of congressional prerogatives, crimes not covered by one of these five categories, and a residual "other" category, acts not punishable as crimes but not fitting into the other categories. The most common charges were campaign violations (N=26), followed by conflict of interest (12), bribery (11), other crimes (11), abuse of congressional prerogatives (9), morals (7), and the residual "other" category (4). Our assumption was that bribery and other crimes would be seen as the most severe offenses, abuses of congressional prerogatives and campaign violations would be seen as least severe, and morals and conflict of interest charges would occupy an ambiguous middle position. We also measured severity according to a variant of the scheme used by Peters and Welch (1978). Categorizing each corrupt act by several criteria, we gave a point for each aspect of the act which is "more corrupt" rather than "less corrupt" (see Welch and Peters, 1977, for a variant of this procedure). Using this scheme we gave each act a score from 0 to 9, with the lower the score the less corrupt the act.

The identification of congressional elections where corruption has been alleged presents the researcher with a very difficult problem. There is no existing catalogue or file of such cases. The present study has relied primarily upon the *Congressional Quarterly's Special Report* on congressional elections which is published in the second week of October during congressional election years.[3] The material for the October 15 issue is collected during September. Therefore, the possibility exists that corruption as a significant issue could emerge and not be reported by *Congressional Quarterly*. For the present analysis we assume that only a few such incidents occur, and if they do, the more flagrant cases will be covered by the national media. To find these cases and to collect the other data necessary to test our hypotheses, we checked the *Almanac of American Politics* (Barone et al., 1974, 1976, 1978), and the indices to the *New York Times*, the *Washington Post*, the *Chicago Tribune* and the *Los Angeles Times* for the years 1968 to 1978.

Methodology. In order to determine the net effects of allegations of corruption on turnout and on vote, we used regression. The Democratic vote in the election under analysis was regressed on the Democratic vote in the congressional election two years before, on the vote four years before, on the control variables, and on a dummy variable indicating whether there was a corruption allegation.[4] We chose to use only the two prior elections in order not to compare present voting patterns with those of more than four years before. Changes in the nature of the constituency might make an analysis using data from elections of six and eight years before somewhat suspect. Data older than that would be difficult to collect because of reapportionment.

The procedure we are using also controls for constituency effects; that is, current direction of the vote is standardized by controlling for the effects of prior votes which also would include the effects of economic and social characteristics of the districts. We also controlled for three further kinds of effects in attempting to assess the impact of allegations of corruption. One set of effects are those short-term forces that are election-specific. Trends in partisanship, while not homogeneous throughout the nation, can be related to the specific electoral context. As a simple example, the Watergate era resulted in Republican electoral losses. One must, therefore, control for the election effect, the short-term forces affecting the partisan vote. To do that, we entered into the equation dummy variables representing each election year (but one).

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[3] The *Special Reports* (such as *Congressional Quarterly*, 1978) analyze the races in each of the 435 congressional districts, focusing on the attributes of the candidates, the important issues in the contest, relevant district facts, and an assessment of the outcome. The synopsis for each election will include an examination of corruption charges if it is a bona fide issue in the race. This means that information on the charges is widespread and has appeared in the local media. Generally, if the congressional race is competitive, several sources of information will be tapped. If the race is one-sided, *CQ* will rely on political news contacts and the clippings from their original search of 80 newspapers.

[4] We chose this method in preference to using a gain score as a dependent variable. Though widely used, a gain score frequently correlates with the base on which the gain is calculated. The reason for this is the "regression toward the mean" effect, whereby cases with larger scores at time 1 are likely to have smaller gains than are cases with smaller scores at time 1. Thus, a negative correlation is likely to emerge between the base score and the gain score. In our data the base score/gain score correlations were quite large and would have substantially influenced the results. (For a discussion, see Bohrnstedt, 1979; Heise, 1970; and Duncan, 1969.)

We also examined the effects of incumbency and seniority. Thus we entered into the regression three further dummy variables: For Republicans, 1 represented a Republican incumbent running in the current election, 0 was coded if there was no Republican incumbent; a similar dummy variable was created for Democratic incumbents; a third dummy variable assigned a 1 to those cases with an incumbent running in one of the past two elections, 0 for all other cases. If there was an incumbent, we also entered the number of terms he or she had served. The median income and the proportion living in the central city in each district also were added to the equation. These variables had no effect and were removed in the final regression. Finally, we entered a dummy variable indicating whether or not the case was one where a corruption allegation had been made. Thus, our regression equations took the summary form:

$$Y_{pv} = a + b_{t-1}x_{t-1} + b_{t-2}x_{t-2} + b_i x_i + b_j x_j + b_k x_k + b_c x_c + e, \text{ where}$$

Y_{pv} = predicted Democratic vote in current election,
x_{t-1} = Democratic vote in previous election,
x_{t-2} = Democratic vote in election four years before,
x_i = election effect dummy variables,
x_j = incumbency and seniority measures,
x_k = income and central city variables,
x_c = whether the case was one of a corruption allegation or not.

In other words, b_c represents the net effect of the corruption allegation on turnout after we controlled for the election effect, previous vote, incumbency and seniority and demographic variables. The regressions used to assess the voting turnout were similar, with turnout replacing percentage Democratic vote in the equation.

Originally, we collected data on each election from 1968–1978. However, in our initial analysis we eliminated cases from 1972 and 1974 because of the mass redistricting between the 1970 and 1972 elections. We also analyzed the data controlling for the partisan direction of the vote in only one previous election. In that analysis we used each election except 1972. The analysis testing the turnout hypothesis used the same procedure.

Findings

Table 1 presents data on congressional incumbents accused of corruption between 1968 and 1978. Defeat in the general election is but one possible fate of congressional incumbents accused of corruption. In addition to determining whether accused candidates met that fate, we were interested in finding out how many resigned from office before the election, how many chose not to run again, and how many were defeated in the primary. We can see that in each election campaign some candidates accused of corruption simply chose not to run for reelection. In recent years, a few candidates also have been defeated in primary elections or resigned from the House. Therefore, even though 75 percent of the candidates who have reached the general election have won, only 62 percent of the accused candidates survive the whole process.

Our analysis will focus on those candidates who did choose to run for reelection and who survived the primary (or were not challenged in the primary). As indicated in the previous section, our analysis also includes a few who were charged with corruption who were not yet incumbents. Table 2 summarizes a few of the more salient features of our 81 cases. As noted in the methodology section, almost one-third of the cases involve alleged campaign violations, while accusations of conflict of interest, bribery and other crimes together constitute another 42 percent of the group. Charges of abuse of congressional prerogatives (such as the franking privilege, overseas travel and the like) and morals charges each account for about another 10 percent, while the remaining five cases are classified as a residual "other" category. Except for a slight decrease in 1972, the number of allegations has risen in each congressional election from four in 1968 to 28 in 1978, about 6 percent of all election campaigns. Most of the allegations involve congressional incumbents, but 16 involve non-incumbents. Most of the allegations had not resulted in official action by election time, though 12 had resulted in an indictment and 9 in convictions. Finally, corruption allegations come from all parts of the

Table 1. Electoral Outcomes of House Members Accused of Corruption, 1968–1978

Electoral Outcome	1978	1976	1974	1972	1970	1968
Resigned during term	0	2	0	0	0	0
Chose not to run again	4	1	1	0	1	1
Were defeated in primary	1	2	2	0	0	0
Were defeated in general	6	3	4	0	3	0
Won the election	16	12	9	3	6	3
Total	27	20	16	3	10	4
Those running in general election who win (percent)	73	80	69	100	67	100
Total winning reelection (percent)	59	60	56	100	60	75

Source: These figures are derived from the *Congressional Quarterly Weekly Reports* (biennial special election reports), and a content analysis of the *Congressional Quarterly Almanac,* 1968–1978 and the *New York Times,* 1968–78. Computations by the authors.

nation, but over 60 percent come from the East and South rather than the West and Midwest.

Table 3 summarizes the net impact of the corruption charges for Democrats and Republicans. These coefficients represent the net loss (or gain) of candidates accused of corruption compared with other candidates and controlling for election effect, prior partisan vote in the district, terms served, and incumbency. We can see that our first hypothesis is confirmed; both accused Democrats and Republicans suffer a net loss in votes at a statistically significant level. However, Democrats lose almost twice as many votes as do Republicans. As predicted, electoral retribution appears to differ widely over the type of offense. Both Republicans and Democrats are punished most severely for morals violations, with bribery the second most punished offense. Abuse of congressional prerogatives, "other crimes," and for Democrats, campaign violations also yield some appreciable voter retribution. On the other hand, conflict of interest charges bring about essentially no retribution, and Republicans seem to suffer little retribution for charges of campaign violations.

By examining the retribution effects for these individual charges, we can better evaluate the difference in retribution for Republicans and Democrats. Part of the difference is that Democrats are more often charged with bribery than Republicans (see Peters and Welch, 1979),[5] and Republicans more often with conflict of interest than the Democrats. Thus, since the retribution for bribery is substantially more severe than for conflict of interest, part of the Democrat-Republican difference is explained. The remaining differences, though, seem more clearly a function of partisan differences. Democrats accused of bribery do suffer a greater net loss of votes than Republicans accused of the same offense; the same is true of campaign violations, the most common charge against members of both parties. The electoral retribution for the other offenses reveal little partisan variability.

We are persuaded by the data in Table 3 that there is an effect of corruption allegations on the direction of the vote. The effects are consistent, very strong for Democrats and reasonably strong for Republicans. The ordering of the retribution among the various charges did not, however, completely accord with our original hypotheses that conflict of interest and "other crimes" would be seen as more serious offenses than they appear to be. Perhaps conflicts of interest are so common as to be almost expected by the voters. Perhaps also, spurious conflict of interest charges, being rather vague, are sometimes levied by opponents in congressional elections but discounted by the voters. At any rate, conflict of interest seems to engender little electoral retribution. On the other hand, morals offenses appear to be punished far more severely than any misdeeds involving money. The electoral retribution even for bribery is less than for morals offenses, but candidates accused of bribery still suffer substantially in their net vote.

What impact does an allegedly corrupt candidate's seniority have on the tendency of voters to support a challenger? We calculated this by entering into the regression equations an interaction variable constructed by multiplying the number of terms an individual had served times

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[5] In the recent FBI "ABSCAM" investigation into bribery charges against eight members of Congress, seven accused were Democrats and only one, Rep. Richard Kelley of Florida, was a Republican.

Table 2. Some Characteristics of 81 Cases of Allegations of Corruption, 1968–1978

Type of Charge	N
Campaign violations	26
Conflict of interest	12
Bribery	11
Other crimes	11
Abuse of congressional prerogatives	9
Morals charges	7
"Other"	5
Frequency by Year	
1968	4
1970	11
1972	4
1974	15
1976	19
1978	28
Candidate's Status	
Incumbent	65
Non-incumbent	16
Candidate's Status at Time of Election	
Convicted	9
Indicted	12
Under investigation	11
Other	49
Regional Distribution of Accused Candidates	
East	24
South	27
Midwest	18
West	12

Source: These figures are derived from the *Congressional Quarterly Weekly Reports* (biennial special election reports), and a content analysis of the *Congressional Quarterly Almanac*, 1968–1978, and the *New York Times*, 1968–78. Computations by the authors.

the dummy variable indicating whether this election involved an allegedly corrupt candidate or not. This computation yielded a variable which had a value of 0 for all the control districts, and a value equal to the terms of office served by the incumbents in each of the "corrupt" districts. Entered into the equation, this term yields the increment of vote for each term of office for those accused of corruption after controlling for all the other variables. The results were that seniority neither increases nor decreases electoral retribution. The regression weights for our interaction terms were nearly zero for both Democrats and Republicans, indicating that for each term served there was a zero increment or loss in expected vote. Seniority brings little protection against electoral retribution in this sense.

In another sense, however, seniority does protect one against the ultimate electoral retribution, i.e., defeat. That is, the degrees of electoral retribution for some of the offenses (Table 3) are quite severe. While most incumbents can stand a loss of 2 or 3 percent in their electoral margin, fewer can stand a loss of 6–10 percent, and relatively few can look with equilibrium on a potential loss of 12–22 percent. Why, therefore, are not more accused candidates defeated when the degree of retribution for some offenses, at least, is fairly large? Of course, some accused candidates *are* defeated. Others are extremely well entrenched, with years of seniority and comfortable vote margins in every election campaign. Obviously, many of the figures involved in prominent corruption scandals of the past few years fit this mode—Charles Diggs, Wilbur Mills, Adam Clayton Powell, Wayne Hays, Edward Roybal, John McFall, Daniel Flood. The evidence confirms the high degree of electoral security of those accused of corruption. In our data, the "average" Democrat accused of corruption has served 6 terms in Congress and in his last election polled over 67 percent. Obviously, electoral retribution of 10 or 15 points will not defeat him. The average Republican is not quite so well entrenched, having served only three terms. But the average Republican vote total in his last election was 58 percent, quite a comfortable margin with which to withstand all but the most severe retribution.

Upon some reflection, perhaps it is not surprising that many of the accused candidates who reach the general election are so well established in their district. After all, an accused candidate from a more marginal district may see the "handwriting on the wall" and choose to retire, or be defeated in the primary. Incumbents with more seniority might be exposed to more temptation for corruption. Then too, incumbents with safe seats and long seniority do not attract strong challengers. Thus accused candidates tend to be more senior and more safe than other candidates. These characteristics protect them from defeat because a greater degree of electoral retribution is needed to defeat them. Further, charges of corruption against well-respected incumbents who have served a long time may engender sympathy—specifically if the charges originate with the federal government. So, while seniority does not markedly increase the amount of retribution one faces, it does provide a larger cushion against retribution than that possessed by the more junior candidate.

Table 3. The Impact of Corruption Charges on Partisan Vote for Accused Candidates (Percent)

Type of Corruption Charge	Net Loss Republican Candidates[a]		Net Loss Democratic Candidates[b]	
All corruption charges	– 6.2	(– 6.1)*	–11.4*	(–11.5)*
Morals charges	–15.4*	(–22.8)*	–22.1*	(–22.7)*
Bribery	–12.8	(–12.8)	–17.5*	(–16.8)
Abuse of congressional prerogatives	– 8.3	(– 7.7)	– 7.2	(– 7.4)
Other crimes	– 6.4	(– 6.7)	– 5.0	(– 2.9)
Campaign violations	– 2.6	(– 0.9)	– 8.8*	(– 6.6)
Conflict of interest	– 1.4	(+ 0.9)	+ 0.3	(– 0.7)
Other	–	–	– 2.3	(– 0.2)
R^2	.65	(.61)	.64	(.61)
N	187	(145)	202	(160)

Source: These figures are derived from the *Congressional Quarterly Weekly Reports* (biennial special election reports), and a content analysis of the *Congressional Quarterly Almanac,* 1968–1978, and the *New York Times,* 1968–1978. The voting data were obtained from the Congressional Quarterly, *Guide to U.S. Elections,* Washington, D.C., 1976, and update supplements for 1978. Data on incumbents and district characteristics is from Michael Barone, Grant Ujifusa and Douglas Matthews, *The Almanac of American Politics* (the 1974, 1976 and 1978 compilations), New York: Dutton.

Note: These figures are the unstandardized regression coefficients for the dummy variable indicating an election where there is a corruption allegation. Election effect, prior partisan vote, terms served and incumbency are controlled. Figures presented are for all elections except 1972. Figures in parentheses are based on all elections except 1972 and 1974.

[a] Includes all control districts plus those where a Republican is accused of corruption.

[b] Includes all control districts plus those where a Democrat is accused of corruption.

*Significant at .05.

Our final proposition concerning the partisan direction of the vote was that more severe charges of wrongdoing lead to more retribution. We tested this proposition in two ways. First, we examined only those cases where individuals had been indicted or convicted of the corrupt charge, believing that if the public paid attention to any charge, they would surely pay attention in cases where the individual had been brought before a court and indicted or convicted. Second, we analyzed acts rated as more corrupt (as discussed in the methods section) by examining separately those acts receiving a score of five or more corruption "points." Table 4 presents the results of this analysis.

Members of both parties do appear to suffer more retribution in these serious cases; the effect on Republicans seems marginal, on Democrats more substantial. Because the number of cases in each category is so small, however, about the strongest statement we can make is that apparently candidates accused of more serious corruption charges suffer more electoral retribution. A more definitive test must wait until a larger body of cases can be collected (Table 4). The other test of the effect of more serious charges proved even more erratic (not shown). In examining only those cases ending in indictment or conviction, we found that the coefficients in many instances ran in the opposite direction than expected. A possible explanation is that sometimes indictments and convictions are easiest to obtain where the act is in fact less serious. Thus, examining only those cases where indictments or convictions have been obtained was not as fruitful as we had hoped.

Summarizing Hypothesis 1, then, we have found that, contrary to much popular wisdom, being accused of corruption does result in electoral retribution. That it does not result in a higher rate of defeat of candidates accused of corruption appears to be related to the greater than average seniority and previous vote margins of those candidates.[6] Seniority does not, however, reduce the amount of retribution in terms of mean votes lost from an expected vote tally. Democrats appear to face more electoral retribution, ranging from practically none at all to losses of over 20 percent. More serious charges within each broad category may result in greater retribution, but this is difficult to prove with the relatively small number of cases under analysis.

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[6] It might be noted that it is certainly not uncommon for a candidate charged with corruption who does manage to win an election to announce his retirement before the next campaign rolls around.

Our second hypothesis dealt with whether these charges affect turnout in the election. We had postulated two possible contradictory effects. On the one hand, charges of corruption might stimulate potential nonvoters to vote in order to "turn the rascal out." On the other hand, corruption allegations might induce potential supporters of the accused candidate not to vote. It is likely that these two effects could cancel each other out. Table 5 indicates that, in fact, corruption allegations have little net effect on turnout. If they do have an effect, it must be examined at the individual level, since the aggregate effects appear to be countervailing. Bribery cases appear to be the only exception to our generalization, but even here the effects are not statistically significant.

We also performed the analysis looking at only the more severe cases as indicated above. Here the effect is somewhat more consistent. More serious charges of corruption seem to stimulate turnout slightly, though only in the case of bribery is there a statistically significant effect, with the turnout increasing 41,462 over that expected. Again, we must not take these figures too seriously, given the small number of cases. Tentatively, however, we can conclude that it appears that more serious charges of corruption will stimulate a net increase in turnout, even though for all charges of corruption the net turnout effects are nil.

Conclusions

Part of a common wisdom seems to be that corrupt politicians continue to "get by" with their misdeeds. This belief is reinforced when members of Congress or other public office holders involved in highly publicized instances of corruption get reelected, sometimes by overwhelmingly large margins. However, our analysis shows that not only do a significant number of accused candidates get defeated at the polls or resign before risking defeat, but also accused candidates suffer a significant loss of votes in reelection bids.

Our findings also reinforce the theory that alleged corruption is, in fact, only one of many factors that voters consider when casting their ballot. The "trading" argument concerning corruption offered by Rundquist and his associates appears valid given our findings here. It has appeal to us because it is able to explain why so many allegedly corrupt candidates do win, contrary to the hopes and expectations of ancient and modern theorists of democracy, yet it does not assume that all or most voters are venally looking after only their own interests. Instead, it argues that a charge of corruption is only one factor that a rational voter will take into account; if the allegedly corrupt act was minor and the candidate has other, overriding virtues in the voter's eyes, such as appropriate stands on the issues and the right party affiliation, then the act will be ignored or rationalized away. Our evidence allows us to infer that voters do take into account corruption charges in making their decisions; some of these charges appear to have almost no electoral retribution, while others are punished quite severely.

If this reasoning about the role of corruption in the electoral decision is correct, it follows that where party identification is strongest or most important in the decision, corruption charges will have less effect in the general election. Corruption charges would also be less influential, it might be reasoned, where there are salient issues in the election beyond the corruption charge itself.

On a more speculative level, our findings raise some troublesome questions for democratic theorists. While being mindful of the fallacy of making inferences about individual-level political behavior from aggregated data responses, we note that some of our findings lead to disturbing conclusions about the role of the democratic citizen and honest government. If, for instance, it is true that Democratic voters vote against allegedly corrupt Democratic candidates in greater proportion than Republican voters vote against allegedly corrupt Republicans, and if the reason for this difference is weaker partisan identification among Democrats, then we can conjecture that weak partisan idenfiers and independent voters have a great likelihood of voting against candidates than do strong partisans. Similarly, our findings provide some evidence for the idea that the more ideological or issue-oriented voters are more apt to "trade" or discount a candidate's possible corruption for valued issues, and therefore are more likely to support an allegedly corrupt politician than those voters who are nonideological and/or non-issue oriented. The disturbing irony for democratic theorists, therefore, is that clean and honest government may depend most heavily upon the action of weak partisan identifiers, non-issue oriented nonideological individuals who display low levels of participation in politics and have less of a commitment to the democratic "rules of the game." But such speculation must await the results of individual-level studies on the effects on the voting behavior of the electorate of charges of corruption against candidates.

Table 4. The Impact of Corruption Charges on Partisan Votes for Candidates Accused of More Serious Charges of Corruption (Percent)

Type of Corruption Charge	Net Loss Republican Candidates		Net Loss Democratic Candidates	
All corruption charges	– 7.7	(– 7.4)	–16.8*	(–19.8)*
Morals charges	–23.3*	(–22.9)*	–28.3*	(–26.9)*
Bribery	–	–	–14.1*	(– 9.2)
Abuse of congressional prerogatives	7.7	(26.9)	–	–
Other crimes	2.4	(5.6)	– 4.6	(7.1)
Campaign violations	4.2	–	–10.9*	(– 3.2)
Conflict of interest	10.2	(12.9)	4.5	(9.5)
Other	–	–	– 3.1	(1.7)
R^2	.65	(.61)	.65	(.61)
N	167	(129)	186	(145)

Source: These figures are derived from the *Congressional Quarterly Weekly Reports* (biennial special election reports) and a content analysis of the *Congressional Quarterly Almanac,* 1968–1978, and the *New York Times,* 1968–1978. The voting data were obtained from the Congressional Quarterly, *Guide to U.S. Elections,* Washington, D.C., 1976, and update supplements for 1978. Data on incumbents and district characteristics is from Michael Barone, Grant Ujifusa and Douglas Matthews, *The Almanac of American Politics* (the 1974, 1976 and 1978 compilations), New York: Dutton.

Note: More serious charges are defined as those where the total score is equal to or greater than 5.

*Significant at .05.

Table 5. The Impact of Corruption Charges on Voter Turnout

Type of Corruption Charge	Net Change in Turnout		Net Changes in Turnout–Serious Charges	
All corruption charges	240	(5,292)	2,848	(12,877)
Morals	–1,313	(2,405)	1,002	(8,538)
Bribery	10,137	(15,650)	16,864	(41,462)*
Abuse of congressional prerogatives	2,261	(8,331)	10,725	(9,335)
Other crimes	3,700	(14,880)	10,376	(45,240)
Campaign violations	–1,068	(1,421)	–1,350	(11,951)
Conflict of interest	–3,811	(1,476)	5,889	(8,425)
Other	–8,217	(339)	1,768	(20,261)
R^2	.72	(.72)	.71	(.72)
N	211	(157)	173	(125)

Source: These figures are derived from the *Congressional Quarterly Weekly Reports* (biennial special election reports) and a content analysis of the *Congressional Quarterly Almanac,* 1968–1978, and the *New York Times,* 1968–1978. The voting data were obtained from the Congressional Quarterly, *Guide to U.S. Elections,* Washington, D.C., 1976, and update supplements for 1978. Data on incumbents and district characteristics is from Michael Barone, Grant Ujifusa and Douglas Matthews, *The Almanac of American Politics* (the 1974, 1976 and 1978 compilations), New York: Dutton.

Note: These figures are calculated with controls for election effect, prior turnout, terms served and incumbency; all elections except 1972 are included. The figures in parenthesis include all elections except 1972 and 1974; in that case turnout in two prior elections is controlled.

*Significant at .05.

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The Rationality of Hegel's Concept of Monarchy

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In this essay I argue that Hegel's concept of monarchy, far from being more evidence of his supposed abstract rationalism or deference to Prussia, is the most complete modern account of the necessary political conditions of a rational state. Hegel believed that the peculiar historical development of European monarchy made the rational state politically possible. Who rules is the fundamental political problem. Some person or persons must have the final power of decision, even in constitutional and parliamentary regimes, and thus stamp the regime with their own particular views. Hegel believed that only the constitutional monarch of a modern state, born to sovereignty regardless of his personal abilities or constituencies, limited by harsh experience and public sentiments to legitimization of public acts, could depoliticize this final power of decision. This alone would allow unimpeded public administration according to rational legal standards. Marxist and technocratic dreams of rational administration will remain mere dreams as long as the dreamers offer no alternative solution to the fundamental political problem.

Hegel's mature political thought combines a breathtaking intellectual radicalism with a recognition, and even grateful acceptance, of historical constraints. In his *Philosophy of Right* he claims to satisfy with a rather conservative form of government his contemporaries' longing for a completely free and rational political order.[1] Hegel told the modern European that there is no need to rummage the past, consult one's conscience or tax one's abstract deductive powers in search of the principles of a free and rational political order; the constitution shared by the developed nations of Western Europe already embodies these principles. Constitutional monarchy is the freest form of government! It is no wonder then that Hegel's systematic political philosophy is the object of so much confusion and the target of so much abuse. The radicals cry betrayal, the empiricists suspect a rationalist fraud, and some liberals, more sympathetic to Hegel, apologize for those ideas in his work that they find distasteful.

The misunderstanding of Hegel's political philosophy is nowhere greater than with his concept of monarchy. Almost all commentators see here at least some signs of either abstract rationalism pushed to an extreme or timorous deference to the existing Prussian government. Often, as in Marx's case, both charges are made; Hegel's pan-logical "mysticism" is said to conceal his dependence on the existing socioeconomic order (Marx, 1971, pp. 20–40). Even some of Hegel's recent defenders, who clear him—successfully, I think—of any serious charge of irrational Prussian nationalism, reluctantly concede that with his concept of monarchy he might be making a slight bow towards Berlin (Weil, p. 60; Taylor, pp. 441–42; see Ritter, 1969, for the best treatment of the various misinterpretations of Hegel's view of the state).

Most readers find constitutional monarchy a disappointing climax to the *Philosophy of Right*. Hegel prepares us for a bold and original political idea by telling us in earlier sections that the laws of contracts, abstract moral duties and the private rights of civil society are all too narrow to contain our freedom, that only in the constitution of the state can we find true political and personal freedom, but then tells us that it is the familiar and somewhat anachronistic form of monarchy which guarantees our complete freedom and the rationality of the state. This is especially difficult to accept in the twentieth century with the disappearance of most of the monarchies that Hegel's contemporaries knew. Hegel understood the difficulty of his conception of monarchy and expected it to be misunderstood. "The concept of the monarch is the most difficult for *'Räsonnement'* [ordinary deductive reasoning], i.e., for the method of reflection of the understanding" (par. 279).

Indeed, it is difficult to see, at first glance, the rationality of hereditary monarchy. Weber has enshrined in theoretical terms the distinction our common sense easily makes between the rational and traditional legitimacy of political authority (Weber, 1964, pp. 324–58). Hereditary authority clearly depends on the public acceptance of a dynastic tradition. Whatever reasons one can use to defend monarchy, its legitimacy, its ability to command willing obedience and respect, rests on the traditional right of one family to special power. Yet Hegel argues that the hereditary monarch is the necessary apex of the rational political order, and he challenges us to understand why this is so. But even if Hegel has a serious argument, if he is neither the rationalist mystic nor the obedient Prussian of literary legend, what relevance does that argument have for us in a period in which the traditional legitimacy of dynastic houses has all but disappeared? Today, when arguments for hereditary monarchy are likely to be based on its value as a tourist attraction, monarchy does not seem to be an important political subject. At best, then, Hegel's argument seems original, but irrelevant.

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[1] The numbers in brackets refer to the numbered paragraphs of Hegel's *Philosophy of Right*. An "a" following the number indicates the additions (*Zusätze*) to each paragraph which Hegel made in his lectures and are collected in the appendices of both Lasson's edition (1911) and Knox's translation of *The Philosophy of Right* (1952).

Yet the concept of constitutional monarchy is the climax of the *Philosophy of Right*, which social and political theorists have found increasingly useful of late. It is Hegel's answer to the problem he shares with liberal and democratic thinkers of the nineteenth and twentieth centuries: how to reconcile the private personal freedom of individuals with the rational and free direction of public affairs. Hegel thought he had an answer to this essential political problem of modern social life. The patience to work through this complex solution is rewarded only if one thinks very seriously about the concepts, like hereditary monarchy, which were crucial to Hegel but seem unimportant to us. By examining Hegel's view of constitutional monarchy, the necessary condition of a free and rational state, we should learn something about our own aspirations towards a free and rational political order.

Hegelian Logic and the Modern Constitution

Before directly examining Hegel's concept of monarchy, we must discuss the philosophic status of his argument for monarchy. Hegel's political ideas may become more accessible when they are abstracted from the systematic character of his arguments, but this method loses the unity and completeness of his political reasoning, which alone make his bold claims sensible. We have to consider the philosophic status of his arguments because misconceptions about it mar most discussions in the secondary literature. His unusual mode of argument is the greatest obstacle to a correct interpretation.

Hegel argues that a constitution is rational when it is inwardly differentiated into three moments (powers)—monarchic, executive, legislative—corresponding to the structure—individual, particular, universal, of the Idea (Concept) of reason (par. 272). (We should note immediately that, for Hegel, a constitution is more than a written document outlining the institutions of a state; objectively, it is all of the historically developed institutions of a nation [*Volk*], while subjectively, it is all of its ethical and political sentiments [*Sitte*]. The totality of these institutions and sentiments, including any written fundamental law, is the constitution [par. 265–68, 273]). Monarchy is rational because of its form, not because of its useful effects. Given this strange disregard for the public utility of monarchy, almost all commentators have treated Hegel's philosophic argument for monarchy as a "deduction" from the "Idea" of reason Hegel develops in his *Science of Logic*. The fullest treatments outline in detail the logical moments of the monarch's function according to paragraphs 275–81 of the *Philosophy of Right* (see Rosenzweig, 1962, Vol. II, pp. 139–46). Many then criticize Hegel for specifying the actual political form of these moments further than his logical structure justifies. One of the commonest criticisms is that, although he can deduce the need for a final, individual power of decision in the constitution, there is no logical necessity that this power be given to a hereditary monarch rather than a president or a ceremonial head of state (Weil, 1950, p. 61; Taylor, 1975, pp. 440–41; Avineri, 1972, pp. 186–87). Hegel is criticized for leaping beyond his logical deduction.

Duncan Forbes (1975, pp. xxx, xxxiv) is one of the few to point out what is wrong with this treatment of Hegel's dialectical reasoning about the state and constitution. "To suppose that there should be strict logical links in the movement of the dialectic is to miss its real value as a device to enable one to think concretely about the state, freedom etc." Hegel does not deduce the specific character of modern monarchy from an abstract set of principles without reference to the actual relationships in the modern state. If his argument were just a logical deduction, one should not expect any correspondence with existing political institutions.

In fact, Hegel rails at those who try to deduce monarchy from a higher principle (par. 279, 281). Ordinary non-dialectical reasoning (*Räsonnement*) can argue for the rationality of an institution only by means of such a deduction. Arguments for monarchy, therefore, usually depend on a hypothetical principle. In the past, as well as for some of the Restoration Legitimists, the higher principle was Divine Omnipotence and Gift or the example of God's monarchic rule of the universe (par. 279). One can then deduce a particular family's claim to rule after establishing a minor premise that the family has received or inherited His Gift of earthly political power or bears special qualifications to serve in God's role on earth. More contemporary arguments deduce a monarch's right to rule from a secular principle; public stability requires the political stability a royal family provides. However plausible these contemporary arguments might be, they can only demonstrate the possible, not the necessary, rationality of monarchy. One can argue in many other ways from the need for public stability, inferring the need for other institutions which might contribute to a stable order. Since a number of different minor premises can follow the hypothetical major premise, the deduction of the family's claim to rule cannot be a necessary argument.

Most of Hegel's interpreters fall into the same error he found in previous arguments for monarchy by treating his argument in terms of ordinary deductive reasoning (*Räsonnement*). They treat Hegel's logical Idea as a set of abstract principles from which Hegel deduces the specific institutions of a modern constitutional monarchy. They treat Hegel's justification of monarchy as a series of "medii termini" from an abstract principle, despite the fact that Hegel specifically rejects that form of argument (par. 281). Hegel does not want to advance *reasons for* monarchy which can be opposed by *reasons against;* he wants to demonstrate the *reason in* constitutional monarchy, its inherent rationality.

What does it mean to speak of the inherent rationality of a constitution? We must consider Hegel's Idea of reason at this point. We will see that, contrary to our expectations, it points us further away from abstract rationalism toward the serious consideration of historically functioning constitutions. For Hegel, the Idea of reason is the structure of relationships in a self-sufficient whole, a unified structure which maintains within its unity all differences. The three moments of this structure are the universal (abstract unity), the particular (abstract difference) and the individual (concrete unity containing difference). A rational structure unifies its opposing moments. Nothing can be left outside of the structure, even if it appears contradictory to include it. Rationality is completeness of explanation for Hegel; a subject is rationally explained only when one can explain its place in the whole.

However one may react to the speculative nature of Hegel's Idea of reason, it is very important to see how he uses it in the analysis of social and political institutions, where it has had its greatest impact. Hegel uses the Idea as a tool with which to understand the complex interdependent relationships within a modern state. He claims that it is impossible fully to understand the actual structure of a modern constitution without it. The modern constitution is an organic unity, integrating different powers and their corresponding institutions. A constitution in which the different powers were simply separate and opposed to each other would not last long before one power would suppress the others or all authority would become ineffective. (Hegel is thinking here about the different French constitutions during the Revolution [par. 272].) Where the separation of powers actually works, there is a unity of spirit informing the different branches of government despite their personal and functional opposition.[2]

Using the Idea as a tool, Hegel can uncover the essential structure which unifies a constitution. It is a most effective tool for him because he also believes that the rational Idea is the underlying reality of all phenomena; however, it is not sufficient in itself to teach one the actual form of the rational political order. In the *Philosophy of Right,* Hegel studies the form in which reason has become actual in the *contingent* circumstances of human social existence. Hegel's logic cannot teach us the rational necessity of hereditary monarchy. We can learn about it only by analyzing the institutions of the modern world in their actual historical context. "It is necessary that the categories of logic should have *an* embodiment in the stages of nature and history, . . . but *what* embodiment is something which cannot be forecast" (Knox, 1952, p. 369, n. 39). Hegelian logic teaches us that it is necessary that reason actualize itself in history; but only a study of history can teach us what form reason takes in the contingent circumstances of human life. Modern constitutional monarchy is not the preordained form of reason in the world. It cannot be deduced from logical principles.

The rational constitution which Hegel describes in the *Philosophy of Right* is incomprehensible and entirely impossible without the development of modern European political institutions, sentiments, morals and manners (*Sitte*), in short, without the development of what Hegel calls the modern German (European) spirit. The Idea and the rational constitution do not form an empty tautology (see Rosenzweig, 1962, Vol. II, pp. 79–82; Fackenheim, 1970). The rationality of the Idea is visible, is actual, only in the interdependent unity of a modern constitution. We must try then to understand why Hegel thinks that monarchy makes it possible for the modern European state to function as a rational whole.

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[2] In this de Tocqueville agrees with Hegel. He argues that a truly mixed government would collapse (1945, Vol. I, p. 270). But while de Tocqueville believes that it is the partisan spirit of egalitarianism which, increasingly, informs all branches of the modern state, Hegel believes that the unifying spirit of constitutional monarchy is non-partisan and, therefore, rational.

Attacking his leaps beyond a narrow logical deduction is, therefore, not an effective challenge to Hegel's political philosophy. To challenge Hegel's theory of the rational constitution, one must either develop an alternative explanation of how a modern interdependent constitutional order functions or show that a particular institution has lost or never fulfilled the function that Hegel prescribed for it.

Monarchy as the Necessary Condition of Impersonal, Rational Rule

The historical development of the modern "German" spirit is the necessary condition of the rational constitution. The essential characteristic of the modern spirit, according to Hegel, is the recognition and spread of private subjective freedom. Subjective freedom is not just the essential concern of modern thought; it has entered into modern social reality. In the socioeconomic sphere, it appears as the ever-increasing differentiation of productive functions, the free choice of careers and the freedom of individual initiative; in political theory, as the contract theory of the state; in law, as the individual's rights of property; in moral philosophy, as the autonomy of moral reason; and in the state, as the inwardly differentiated constitution (par. 272). The modern constitutional monarchy develops and reflects these conditions of released and recognized individual subjectivity. The modern monarchy has been developing, according to Hegel, since the Reformation, but it has begun to mature only in the post-Napoleonic regimes of England, France and, especially, Prussia. It is not a universally applicable model because it depends on historical conditions which cannot be duplicated at will. It is still, however, *the* rational and free regime. Although every nation has the constitution appropriate to it (par. 274), the fully developed monarchy is the best constitution.

Hegel's standard for ranking constitutions is the level of freedom they contain. The freest state is the best (Hegel, 1974, pp. 652–54). Since freedom is rational self-determination for Hegel, the free state is also the rational state. Freedom has two sides: the individual's choice of his own vocation, and the rational organization and direction of the will of the community (par. 257). We will call them *subjective* and *substantial* freedom, respectively. Substantial freedom is true self-determination, because with it a specifically human will, the historically developed rational will of the state, replaces mere natural determinants—instinct, inclinations, chance, etc.—of the human will (see Riedel, 1974). To possess substantial freedom the individuals must recognize the will of the rationally directed state as their own (par. 257). In the truly free state, substantial freedom will promote and protect subjective freedom, while the subjective self-determination of individuals will not oppose their identification with the community. Hegel claims that subjective and substantial freedom are interdependent in the modern constitutional monarchy.

This claim is difficult to understand because, at first glance, the two freedoms seem mutually exclusive. Most would agree that modern liberal constitutions aim at protecting the private rights of individuals, but argue that this weakens the identification of the individual and collective wills. Rousseau made his great protest against the selfish private character of modern life in the name of the substantial public freedom he saw in the ancient, participatory republics of Greece and Italy. Hegel recognizes, along with Rousseau, the ancient republics as the historical model of substantial freedom; but he also saw a fatal deficiency in their freedom. Ancient citizens ruled themselves directly or through elected leaders in their participatory republics; but they were not really free, according to Hegel, because the content of their public decisions was left to chance (par. 279). Lot or election or nobility were the requirements of public office, none of which guaranteed that the man who filled the office would have the appropriate abilities. The ancient states, therefore, depended very much on the good fortune of having leaders who really had great virtues. When a Pericles is not at the helm, the state functions badly, if at all. The constitution of the ancient republics required leaders of great talent, but could not guarantee their continued presence. Thus the direction of affairs in the ancient republics was left to chance, although they appear to follow the free will of the nation. Man does not rule himself in the classical republic. He has not yet replaced dependence on the contingencies of nature—in the talents and character of individuals—with the guidance of a rationally organized collective will. The need to consult oracles and take the auspices in order to confirm political decisions, reflects this ruling contingency in the ancient world. There is no adequate form of rational self-determination in the ancient world. The ancients, therefore, do not recognize the legitimacy of complete self-determination; political decisions require some form of natural or divine confirmation (par. 279, 279a). They must reflect the natural order or a divine will as well as a human decision. Free self-determination of all is characteristic only of the modern world. Only a state in which political decisions do not depend on the abilities and character of particular individuals has a rational and, therefore, free constitution.

Hegel notes quite often in the *Philosophy of Right* that Plato recognized this problem with the ancient state, but that its immature historical development limited the rationality even of Plato's imagined republic (par. 184a, 185, 206, 262a). Plato's solution was the direct rule of reason in the person of the philosopher-king. The philosopher-king, freed of traditional prejudices, could put each citizen in his truly appropriate place. This might satisfy the demands of justice, which was Plato's concern, but not the demands of freedom, according to Hegel. To put each in his proper place, the philosopher-king would have to deny the individual's free choice of vocation as an irrational and dangerous luxury (par. 185, 262a). Even the philosopher is not allowed the free choice of vocation in Plato's just city; he must be compelled to rule (*Republic*, 519b–e). Furthermore, Plato admitted that the rational direction of political affairs is subject to chance, the very unlikely chance of the coincidence of philosophy and royal power (*Republic*, 473d–e). Reason necessarily rules the state only when the state's will does not depend upon the ability of particular individuals, be they philosophers, heroic leaders or technocrats.

Hegel believes that the modern hereditary monarch ensures that the different institutions of a modern state function together as an interdependent rational organism. The three powers of a modern constitution have distinct functions, but are not completely separate. As part of an interdependent whole, each power is defined not only by its own particular function, but also by the other powers which limit and interact with it. Each power of government reflects the others (par. 272). A change in the function of one power changes the nature of the others. The constitution thus works as an organic unity. Hegel uses the organic metaphor to illustrate the genuine interdependence of the institutions of a modern constitution. (We should not confuse its meaning in Hegel's work with its more traditional use in political literature, as in Menenius Agrippa's fable of the belly, as a justification of a hierarchical order.) The legislature refines the particular opinions of the individual members of civil society in discussing and approving laws, bringing individuals to an appreciation of the universal; the executive officers apply the universal determinations of law to particular cases; and in the monarch's will "the different powers are bound into an individual unity which is thus at once the apex and basis of the whole, i.e., of constitutional monarchy" (par. 273). The monarch's will unifies and legitimizes the work of the different powers and officials as the work of the one individual free will of the state. What are we to make of this? It is easy to understand the monarch's unifying function in a symbolic manner. As such a symbol, it has a role which will be discussed below. But Hegel argues that the monarch's function is not just symbolic, and we have argued that the monarchy is not just the symbol of a logical unity. We must closely examine the monarch's power of final decision in the context of the entire constitution.

Hegel argues that the power of final decision, of political sovereignty, has to have a place somewhere among the powers of a constitution. Talk about the sovereignty of the people has a fine ring, but the popular will is, in itself, nothing but an abstraction; it receives a determinate form only in actual political institutions. The sovereignty of the state is the real meaning of the sovereignty of the people (par. 279). Unlike "the people," the state is not just an abstract concept; its historically developed and recognized institutions give it a concrete existence. The will of the state is the only concrete manifestation of the will of the people, according to Hegel (par. 279). The concrete unity of the state's will is explicit in its relations with other states. In foreign policy, the state, directed by those with the final power of decision, undertakes action as a collective individual among other such individuals. Internally, the state's will is an implicit unity, since it is only their accordance with the recognized constitutional order of the state which gives the various domestic institutions, laws, judgments and officials their legitimate authority.

In the *Philosophy of Right*, Hegel tries to prove that the state's will is the concrete manifestation of free human will in the world. In the first two parts of the book, "Abstract Right" and "Morality," he argues that the most obvious manifestations of human freedom, the self-imposed contractual and moral obligations of individuals, are, in fact, abstractions which gain concrete significance only in the historically developed institutions and sentiments of a nation. The state's will is thus the concrete free will of a people.

Who rules, who has the final say in deciding what the content of the state's will shall be, is therefore, *the* fundamental political question. Any particular individual or group of individuals, elected or self-appointed, is bound to be partial and not represent the universal will of the nation, even if only because they must depend on their own limited insights. For this reason there exist all of the constitutional means of removing the ultimate power of decision from the hands of particular individuals and placing the final power of decision in a body of fundamental law, as in the United States, or with an artificial collective will, like that of the British Parliament. The problem with all such constitutional devices is that, while providing greater stability by moderating the consequences of political competition, they do not end the personal competition for the ultimate power of decision in the state. The popular will embodied in a constitution or a parliament is, in fact, an artificial will, and is recognized as such (par. 279). We look for the real persons responsible for the hundreds of things done each day in the name of the state, and individuals compete for a share in that responsibility. The president is not the sovereign of the United States, yet we are right to hold him responsible for a large part of the government's activities. He is an elected leader who uses his particular opinions and talents to decide the policy of the state. The content of the state's will is, to a great extent, contingent upon his particular abilities and virtues. Constitutional and parliamentary democracies are partly subject to the same chance which ruled the ancient republics.

The rational constitution is possible, for Hegel, only where the ultimate power of decision is *in the hands of a real person* whose impartiality is certain, and beyond whom there is no higher personal authority to appeal. Only the modern constitutional monarch can fill this role. Hegel argues that the essential function of a modern European monarch is to legitimize each act of state by adding to it the authoritative approval of the sovereign. His qualification for the job is his birth into the ruling family; his right to rule does not rest on any particular abilities. Nor does he depend on the will of any other individual for his position, as governors-general and ceremonial presidents depend on elected party leaders for their power. The acceptance of primogeniture succession completely removes the crowning of a modern monarch from the sphere of human choice. The monarch owes his position only to nature.

The monarch's impartiality is a little more difficult to explain. It is important to realize, at once, that the impartiality of Hegel's monarch is not that praised by sycophants of absolute monarchs throughout the ages. Compare the following argument of a Byzantine jurist, recorded by Gibbon, with Hegel's. "What interest or passion can reach the calm and sublime elevation of the monarch? He is already master of the lives and fortunes of his subjects, and those who have incurred his displeasure are already numbered among the dead" (Gibbon, n.d., Vol. II, p. 322). That is how a toady of the court speaks. Hegel speaks of a monarch whose main function is to say yea and dot i's (par. 280a, 279a). Hegel's king is no absolute monarch. Indeed, if he acts according to his own particular ideas he is, by Hegel's definition, a despot, since constitutional rule is essentially impersonal rule (par. 278). The entire substantial content of the monarch's will "is absolutely dependent" upon the law and its application by the executive. The rational functioning of the state does not depend on the contingent character and talents of the monarch; anyone could do the job (Hegel, 1974, pp. 678, 685).[3] In his lectures on art Hegel points out that contemporary monarchs are not suitable subjects for artists because of the complete disjunction between their functions and their personal qualities (Hegel, 1975, Vol. I, pp. 193–94). It is not even important that the monarch choose the best candidate for each job when making appointments to the executive. Ministerial and high civil service posts require competence, not personal genius; given a number of objectively qualified candidates, it is not essential that the monarch choose the best (par. 292).

The preceding paragraph shows only that the monarch need not exercise his particular judgment in order to perform his role in the constitution; *it does not prove that he cannot if* he wants to. It seems that the functioning of the constitution still hinges on the contingent character of an individual. This would be a sound criticism if Hegel had really based his concept of monarchy entirely on an unhistorical logical deduction. In fact, Hegel's concept of monarchy depends on the historical development which has led to the actual limited role described above. European monarchs were not always so hesitant to assert their particular views; but over the centuries, and not without a few very hard knocks, their more limited nineteenth-century role had developed. The impartiality of monarchy is the work of history, not a gift of character.

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[3] Although Hegel's stronger comments about the mere formal character of the monarch's function were made in his lectures and not his published works, one cannot accuse him of cowardice, since those lectures in Berlin were often attended by high officials of the court and civil service.

With the monarch limited to the mere formal moment of subjective decision, it seems that the constitution is a monarchy only in name, while, in fact, it is a bureaucracy, the rule of public officials. Hegel admits that the monarch's will is a formal will, a mere name on governmental decrees; but he insists that "this name is important" (par. 279a). It makes a rational self-determination of the state's will possible.

The modern ideal of the rational state, in both its technocratic and Marxist forms, concentrates on the replacement of politicians with knowledgeable administrators, who perform limited functions rather than compete for power; but its proponents usually ignore the necessary political preconditions of such a rationalized social order. Dreams of politics as rational administration will remain mere dreams as long as the dreamers cannot solve *the* fundamental political problem—who has the legitimate authority to rule?—in a way which will not disturb the impersonal rational direction of public affairs. If it is open to debate which person has the final power of decision, competition for that power will exist and continue to distort rational administration with competing personal and partisan influences, whether the regime is a democracy or a bureaucracy. Placing ultimate sovereignty in an impersonal representation of the popular will, such as the American constitution, does not end this competition and partisanship. In fact, the American constitution protects it. Competition for power and partisan influences continues just as obviously, if less honestly, at the top of the Communist party bureaucracies, despite the theoretical derivation of their legitimate authority from the general will of the workers and peasants. As long as the final decisive power is available in a state, individuals will compete for it.

Hegel believes that monarchy, as practiced in early nineteenth-century Europe, can solve the political problem satisfactorily. The monarch's generally accepted position as the sovereign, whose will legitimizes the acts of the state, places a severe limit on the ambitions of bureaucrats and politicians. Partisan groups and personal interests may continue to exist in the government, but they will not direct it from the top. At the same time, the monarch does not give the regime a personal partisan direction because his role is limited to the final legitimizing approval of laws and public actions. By depoliticizing (or depersonalizing) the regime at the top, the monarch creates the conditions in which freedom of political association and competition can continue without disturbing the general rational administration of the state (Hegel, 1964, p. 160; 1913, p. 27).

Hegel's constitutional monarch defuses the competition of individuals to make the final decisions according to their personal opinions. The power of the constitutional monarch is, to use Benjamin Constant's term, the *"pouvoir neutre."*[4] It legitimizes and unifies the diverse acts of state, but initiates no particular actions of its own. The rational state requires a depersonalized power of final decision, exercised by an actual person. The fundamental problem of politics cannot be wished away. Someone must have the final say in any human society. Hegel never believed that the scientific administration of things would replace the political rule of man over man, as did his most famous wayward disciple. Social problems ultimately require *political* solutions. But he did believe that the peculiar development of the political powers of the modern constitutional monarch made the rational direction of state and society possible.

This is a solution to the problem of rational government so strikingly original, so tantalizingly close to the practice of actual states, and so consistent with his general interpretation, as well as with the basic realities of social life, that we can, perhaps, forgive Hegel his failure to discuss the major problems of how such a rational regime would function in practice. (He says much less about the role of the bureaucrats, the rational administrators in the executive branch, than about the monarch in the *Philosophy of Right.*) Hegel incorporates into his systematic thought the paradoxical revelation that the most traditional element of the modern constitution makes the rational regime possible. The most personal form of sovereignty makes impersonal, rational rule possible. Political thinkers who treat monarchy as an institution apart from its place in a developed constitution could never grasp this paradox. Hegel believes that only his dialectical reasoning can uncover and help us to understand such complex, interdependent social structures.

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[4] Constant's argument for monarchy is quite similar to Hegel's. Constant, however, was only interested in protecting individual freedom (1978, p. 1101); he was highly suspicious of the longing for the substantial political freedom of the ancients. He does not argue, as Hegel did, that constitutional monarchy gives citizens substantial collective freedom.

Monarchy is the rational form of government only under very specific historical conditions. The rationality of hereditary monarchy is not based on a deduction from higher principles which would establish the universal validity of this one form of government. It is rational only in the context of the actual institutions and political sentiments which have developed in modern Europe. Considered apart from them, hereditary monarchy has no special claim to rationality. In itself, the claim of the firstborn to the throne is an accidental and irrational claim to office (par. 280). Hegel, himself, calls the modern acceptance of primogeniture succession a return to an earlier principle associated with immature forms of government, but now as part of a fully developed constitution (par. 286). Ordinary deductive reasoning sees only the irrationality of honoring the accidents of birth, and thus fails to understand the rationality of primogeniture succession in a modern constitution (par. 281). Choosing among a large number of sons, as the Chinese emperors did, seems, in itself, to be a more rational procedure; at least the successor might be the best of the brood. It is, however, its very arbitrariness that recommends primogeniture succession to Hegel. A rationally integrated constitution does not need a monarch with special talents and virtues, so that a large pool of princely candidates is only a source of internal dissension and instability. A rational constitution only requires that the monarch take the trouble to be born. Modern citizens who echo Figaro's outrage at the irrationality of this qualification for high office are thinking superficially, according to Hegel. Primogeniture succession institutionalizes, in a publicly acceptable manner, the depoliticization of the ultimate power of decision in the constitution.

The recognition of the justice of primogeniture succession is entrenched in the ethical-political sentiments of the modern European nation, even though the accidental distinctions of birth are not a recognized principle of justice throughout the modern constitution. Its utility is not the basis of this sentiment; Europeans feel that skipping over the firstborn would be wrong. (It would be like pushing ahead in a queue, in a country where queues are respected.) It is an ethical sentiment, part of the *Sitte* of the modern European nations, not a collective agreement about the best means to a particular end. Hegel's limited constitutional monarchy is unthinkable without it.

How can we explain the existence of the sometimes contradictory combination of ethical sentiments upon which the rational constitution rests? Hegel answers with a poetic outburst about the "inner might of Spirit" actualizing itself in the contingent circumstances of human life (par. 281a). The sentiments and institutions of a modern European nation fit so neatly together in a rational whole because they are the actualization of the Spirit which underlies all existence. Hegel believes that one can explain the strange conjunction of just the right conditions for the rational constitution in the modern state only by referring to a deeper reality, his concept of Spirit. Hegel, however, does not deduce his model of the rational constitution from the ontological-logical structure of Spirit; rather, he uses the logical structure of the Idea to analyze the actual existing unity of the constitution of the modern state, and he uses Spirit's self-actualization to explain why the combination of institutions and sentiments appropriate to the rational constitution is a necessary development of freedom in the world.

The key to the existing rational structure of the state is the unifying subjective will of the monarch. There is no mechanical guarantee that he will keep to his precise role. Hegel is in favor of written constitutions defining and limiting the different powers of the state; but to him such a constitution is just a piece of paper if it does not correspond to the actual sentiments and historical preparation of a nation (par. 274). The real constitution of a nation is the totality of its political institutions, laws, ethical sentiments and traditions: the spirit of the nation. The spirit of modern nations decreases the need and expectation of an assertive individual ruler. (Hegel thought, a little too confidently, that Napoleon was the last needed to fill that role, and that his actions, leading to the reform of European constitutions, made it unnecessary in the future.) Consequently, the monarch will usually fit the role that the spirit of the nation demands. The Turkish emperor is a rightful monarch for the Turks, but not for the Greeks with their "milder" European manners (Hegel, 1974, p. 683).[5] It is not entirely accidental that someone like Queen Victoria ruled England for the better part of the century, rather than a monarch with Napoleonic pretensions. The true guarantee of the monarch's behavior is the interdependent unity, the spirit, of the modern constitution. Each institution, official function and political sentiment determines and reflects the others, including the monarch's role. Monarchs cannot choose their own limits; these limits are determined by the customary relationships between institutions which are generally recognized as fitting.

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[5] Hegel shared the delusion of the Philhellenes that the *Sitte* of the modern Greeks were "softer," that is

The Monarch as the Guarantee of Public Freedom

It should be clear now why Hegel thinks that constitutional monarchy is the rational constitution of the state. But it is less clear at this point how a hereditary constitutional monarch guarantees substantial public freedom. Rational administration according to legal standards can protect private subjective freedoms by maintaining the regular and impartial application of the laws of contract and property, but so could the power of an absolute monarch. Hegel claims that the citizens of a modern state possess a substantial political freedom, as well as the free choice of career and private interests. How is this the case if the sovereign owes his power to his birth, and his appointed ministers are not responsible to the representatives of the people?

We have already seen the inadequacy of the ancient republican ideal of political freedom. Direct or indirect popular rule is always, to a certain extent, the rule of chance. It is the rule of particular individuals with contingent abilities, not free self-determination. The citizen gains true public freedom only as a member of a state whose institutions remove the chance elements of particular character and natural inclinations from the final decisions of the state. The monarch thus guarantees the substantial freedom of the citizens by guaranteeing the rationality of the constitution. For Hegel, political institutions embody the actual degree of freedom of a nation. Modern constitutional monarchy is the freest form of government because its institutions blend substantial and subjective freedom into an interdependent unity (par. 257–58).

It is still not clear, however, why the citizen of the modern state should identify his will with the state's will, rather than recognize it as a foreign, if useful, imposition. If the rational will of the state is an imposition, then the citizen will not be free, and the state's will cannot be the free self-determining will of the nation. In Hegel's monarchy, the citizens find some recognition of their particular opinions through their elected representatives in the legislature and their participation in the corporations; but it is again the monarch who firmly binds the state together. The monarch unifies all of the various actions of the state under his will. He is the concrete representative of the nation's will. He is the natural individual who freely wills the universal public good; he is the very image of free, rational self-determination. The disjunction between his particular qualities and his function, the fact that any man could do his job, allows the citizen to identify with his will, and thus with the state's will.

It is much harder to identify the universal will of the nation with elected or appointed figures. They serve because of their particular opinions and abilities, of which one may or may not approve, and which remind one that they bring personal ambitions and perspectives to government. When citizens do identify the universal will of the nation with the will of a non-hereditary leader, grave dangers can arise, because in order to value the opinions of a particular individual as more representative of the real will of the nation than the political institutions of a developed state, they must have an unquestioning faith in his privileged personal insight. Given a choice between the penetration of particular interests into the leadership of a liberal democracy and the irrational charismatic unity provided by the leader's cult in a fascist state, I have no doubt that Hegel would enthusiastically embrace the former; but he argued that the modern constitutional monarch opened a path between the rule of combinations of particular interests and irrational universalism.

Citizens can identify with the will of the monarch precisely because his particular opinions are irrelevant. His only relevant quality is his birth into the ruling family. Like every other citizen, he is a member of a determinate nation. Abstract individuals without determinate qualities do not exist; there are only Englishmen, Germans, etc. A nation's constitution, to a large extent, impresses those determinate qualities on its citizens. By identifying

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more European, than the Turks'. The Greeks, however, were still Christians, and thus from a Hegelian viewpoint their faith contained the principle of free subjectivity, even if that principle did not yet determine their behavior.

with the monarch's will as the will of the nation, the individual recognizes *himself* in the constitution.

The monarch clothes the rational state with the glow of majesty (par. 281). Without it the will of the rational state is the lifeless imposition of the bureaucracy. The monarch's majesty stems from the mysterious and accidental unity of a particular individual and the fate of the entire nation. The monarch's individual will is the universal will of the state. For Hegel, the reverence for the monarch is, in essence, the reverence for the human capacity for rational self-determination, the true freedom which the individual possesses as a member of a rational state. By making it possible for the individual to identify with the universal will of a rationally organized state, the constitutional monarch allows him to recognize and revere his own true freedom.

Since it requires a tradition of accepting a royal family, as well as the historical experience and political conditions of post-Napoleonic Europe, Hegel's rational constitution is not a universally applicable blueprint for the best regime. It would have been absurd for the authors of the American constitution to try to impose this form of government on a country without a native, hereditary aristocracy, from which to choose a royal family, even if they had done their work in 1825 and acknowledged Hegel as their master. The sad fact is that the rational and free constitution is *not* possible everywhere. Hegel was not the blind optimist he sometimes appears to be in the textbooks. He argued that rational principles of government were spreading; but he never claimed that the rational state would become universal and perpetual.[6] Some states might develop and lose the rational constitution; some might never see it. Hegel knew that politics would still be a cause of human misery in the future.

The rational constitution, according to Hegel, is not yet completely realized in any particular country of Western Europe, including Prussia. (A comparison of the institutions described in the *Philosophy of Right* and those of Prussia in the 1820s quickly reveals important differences; see Hočevar, 1973.) The rational constitution might best be thought of as a combination of the best elements of the three most fully developed European states, a combination of English subjective freedom, the French Charter and the Prussian legal code and civil service. Any particular state has defects because of the unavoidable contingencies of all actual historical development (par. 258a); but one can still find the essential principles of the rational constitution functioning in an actual constitution. Hegel tries to identify the rationality in the modern state which these contingent defects hide from most observers.

Hegel does not rule out reform of those defects in particular constitutions; he only fears that rational constitutional reform is a far more difficult and dangerous business than is generally understood. Reformers are likely to make the same mistake that most interpreters of Hegel's concept of monarchy have made. They tend to look at the rationality of a particular institution in abstraction from its function as a moment of an organically integrated constitution. Monarchy will thus seem to be an irrational anachronism in the modern constitution. Reformers are likely to do more harm than good. Even when Hegel recognizes that a particular flaw cries out for reform—he calls the tangled web of private rights in the English common law an "Augean stable"—he argues that reformers tend to produce havoc by introducing principles which contradict the spirit of the constitution (Hegel, 1964, p. 310; 1913, p. 30). The kind of intelligent reform which he favors is very difficult to achieve.

As for revolutionary change, it may be necessary in those parts of the world where the constitution still does not recognize the essential modern principle of subjective freedom, but not elsewhere. This is not to say that Hegel believed that further revolutionary change is impossible in Western Europe, but only that he believed no revolution could bring about a higher degree of freedom than that found in the modern constitutional monarchy.[7] Many may still long for what they believe is a higher form of freedom, for a more direct democracy, for the abolition of property, the abolition of political authority, for local self-determination, etc., but further revolutionary change will always one-sidedly favor either the subjective or substantial side of freedom, and thus result in a net loss of freedom. True freedom is the integrated balance of substantial and subjective freedom which constitutional monarchy provides.

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[6] We cannot settle the argument about the "end of history" in Hegel's thought in a footnote. We can say, however, that just because the essential task of human history, the actualization of freedom in real political institutions, is complete does not mean that nothing else of importance will ever happen; it only means that, whatever happens, there will be *no more complete development* of freedom. There still will be war and disorder in the world; the end of history does not bring the end of human suffering.

[7] Anarchists exaggerate the extent of individual freedom, while revolutionary nationalists and Jacobins exaggerate the sphere of substantial freedom. Marx accepted Hegel's ideal of freedom as the blend of substantial and subjective freedom, but he offers no political means of institutionalizing it. It is, therefore, not surprising that in the communist states, which are based on Marx's dream of the replacement of the rule of man over man with the scientific administration of things, the final power of decision rests, in fact, in the hands of one or a few individuals whose virtues and

Conclusion

Constitutional monarchy solves, for Hegel, the crucial political problem of the modern state: how to reconcile the subjective freedom of individuals with the free and rational direction of public affairs. Hegel is optimistic about the prospects for the rational direction of the modern state because the historical development of the European monarchies has made the solution possible. It is not surprising that Weber, writing after the transformation or disappearance of the institutions and sentiments which were crucial to Hegel, is pessimistic about the possibility of a rational regime. He can see no realistic means of depersonalizing the final decisions of the state. The bureaucracy may conduct everyday affairs according to rational legal standards, but passionate individual leaders must make the final decisions. Thus the direction of the modern state often depends on the contingent character and talents of individuals (Weber, 1958, p. 96). Hegel, on the other hand, believed that the rational state was possible because, given the constitution and underlying spirit of the modern European states that he knew, monarchy offered a concrete political means of depersonalizing the politics of the state.

Yet, after all of the corrections and qualifications of the traditional views about Hegel's political thought that we have made, it is still true that Hegel was wrong about the direction in which modern European states and political sentiments were developing. He underestimated the political consequences of the modern emphasis on individual subjective freedom, and he failed to predict the powerful hold that nationalist sentiments would begin to have on modern politics in coming decades. Europeans demanded a more satisfying recognition of their own freedom than constitutional monarchy could provide. They asked for self-determination, either as a majority of individuals or as a unified nation. Liberal reformers argued that only the subjective choice of the majority of individual citizens could legitimize the authority of any leader, even if that meant dependence on the chances of popular election and the virtues of particular leaders. On the other hand, the abstract nation, rather than the developed concrete institutions of the state, came to represent the substantial unity of a people. The irrational emotional appeal of nationalism proved more powerful and satisfying than the moderate attachment to the institutions of constitutional monarchy which Hegel counted on; it soon warmed far more European hearts than the glow of majesty. With these changes in political sentiments, the delicate balance between substantial and subjective freedom, which Hegel believed that constitutional monarchy maintained, tilted one way or the other. Hegel's hopes for the continued development and spread of the rational constitution foundered on the rocks of the growing individualism and nationalism of the nineteenth and twentieth centuries.

But if we have lost our monarchies and/or the sentiments which supported them, what possible relevance does Hegel's concept of monarchy have for us? His view of the rational constitution is not incorrect simply because it is not possible in our times. To refute his view, one would have to demonstrate that a rational and free regime is possible under other actual historical conditions, or that it is not possible under the conditions Hegel describes, or that it is not possible under any imaginable realistic conditions. Marx and his followers, inspired by Hegel with the expectation of a completely free and rational order, have attempted the first refutation, but without offering an alternative *political* means of depersonalizing politics. They leave the most important problem untouched. It should not be surprising then that communist states are, in fact, ruled by the personal will of one or a few individuals, whose particular abilities and virtues are tremendously exaggerated in public propaganda.

Hegel's concept of constitutional monarchy is the most fully developed modern account of the necessary political conditions of a rational state. He teaches us that we cannot wish politics out of existence with our longings for a rational and free political order. Social revolution cannot establish that order by itself. The social problems of man require *political* solutions. Hegel teaches us that the rational has

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abilities must be tremendously exaggerated in order to justify their rule.

become actual, but only under very particular historical and political conditions, which no longer entirely apply to the modern state. He can teach us to resign ourselves to those limits to our social aspirations which we cannot remove, and how to recognize the conditions under which the demand for more freedom and uncompromising rationality will only bring us less of both.

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Motive and Goal in Hannah Arendt's Concept of Political Action

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Hannah Arendt's work is of major importance primarily because of the categories of thought she originates, especially her concept of political action. But this concept has frequently been criticized for being irrelevant to, or incapable of comprehending, strategic concerns. This criticism, however, is based on a misreading of Arendt on the relationship of specific motives and goals to political action. The critical interpretations of three commentators are considered here: Kirk Thompson, Jürgen Habermas, and Martin Jay. A detailed explication of the relevant texts from Arendt demonstrates the misreading of Arendt on which these criticisms are based and at the same time reveals the subtlety and power of Arendt's conception of the relationship between instrumentality and meaning in political action. Once this relationship is correctly understood, it becomes possible to appreciate the implications of Arendt's work for questions of political strategy.

The seminal thinking of the late Hannah Arendt will, I suspect, come to be seen as one of the major twentieth-century contributions to the history of political thought. The importance of Arendt's work lies primarily in the categories of thought she originates, especially her concept of political action. One indication of the power of her concept of politics is its potential for illuminating our political present as well as some possible paths from that present into a better political future. But unfortunately a serious misinterpretation of her position obscures the light which her thinking can shed on questions of political strategy. Indeed, her work is frequently criticized for being irrelevant to, or incapable of comprehending, strategic concerns. This criticism is based, I believe, on a misreading of her discussions of one crucial facet of politics: the relationship of specific motives and goals to political action. It has been argued that Arendt insists on a radical split between politics and all instrumental activity, that she defines politics as devoid of instrumental concerns. Thus she is seen as proposing an understanding of politics that is divorced from the cares and concerns of the real world. Closely related to this point is the contention that she insists on an absolute split between politics and economics so that true political activity must be free from all economic interests. In fact, the belief that Arendt conceived true politics as devoid of strategic and economic concerns appears to be widely accepted. I will argue that this interpretation arises from a misreading of the relevant texts and, furthermore, that it obscures one of the most powerful and subtle of her insights: her account of the relationship between instrumentality and meaning in politics.

The structure of my argument is as follows. In the first section I present a preliminary and partial explication of Arendt's understanding of politics which reveals those aspects of her conception that the critics have focused on and the basis, insofar as there is one, for their charge that she sees true politics as devoid of motives and goals. The second section introduces three examples of criticisms of Arendt on this point as they have been advanced by Kirk Thompson, Jürgen Habermas, and Martin Jay. Part 3 is devoted to a more thorough analysis of Arendt's texts on this issue, presenting what I take to be a correct reading of Arendt on the relationship between instrumentality and meaning in action. This detailed textual explication reveals the subtlety and power of Arendt's conception of the role of motive and goal in political action and provides the basis for an evaluation of her critics. In the fourth section I proceed to an evaluation of the critiques advanced by Thompson, Habermas, and Jay. And finally, in the fifth section, I discuss some of the strengths and weaknesses of Arendt's

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position and the relevance of her conception of action to questions of political strategy.

Arendt's Concept of Politics: A First Look

Hannah Arendt's political thought is both eclectic and original. By her own account she employs a simple method: "What I propose, therefore, is very simple: it is nothing more than to think what we are doing" (1958, p. 5). This method leads her to focus on the meaning of the human experience of "living-together" for those involved, and by focusing on the historical forms of human living-together she develops a set of fundamental categories in terms of which one can understand human history. The human world of meaning is created out of the natural world by acts of human freedom, and Arendt's concept of freedom is based on an existentialist understanding of man. Man is a self-determining or self-creating being; there is no given and immutable human nature. But human freedom is political for Arendt in that it is never a purely individual or private affair. Freedom is a matter of human association, and while some associations foster freedom, others destroy it. Freedom is not guaranteed man by nature.

The Greek city-state, in particular Periclean Athens, represents for Arendt the first historical appearance of the political association, which is to say that the *polis* marks the beginning of the Western tradition of politics. The rise of the *polis* gave to the Greeks a political life sharply distinguished from their private lives; it added a realm of common concerns to the pre-existing private concerns of the household (1958, p. 24). For Arendt the city-state is the original instance of the "right-ordering" of the life of action; the life of the city-state is based on a correct understanding of the relationships among the three categories of human activity: labor, work, and politics. This understanding finds its expression in the distinction the Greeks drew between the life of the household (the realm of natural necessity) and the life of the city (the realm of freedom).[1]

In developing her own conception of political action, Arendt identifies it as one of the fundamental modes of human activity. The *vita activa* comprises three basic activities which correspond to three fundamental conditions of human life. *Labor* corresponds to the condition of biological life itself. Man as laborer, or *animal laborans*, is submerged in the biological life process. Since laboring is essential to sustain biological life, *animal laborans* operates in the realm of necessity.

Work corresponds to the condition of worldliness, to the unnaturalness of human existence. While not unrelated to the biological life process, work is not embedded in it. *Homo faber* rescues *animal laborans* from the endless flow of biological life and transcends his own subjectivity by constructing a durable world of his own which stands apart from both the maker and the natural world. But "during the work process, everything is judged in terms of suitability and usefulness for the desired end, and for nothing else" (1958, p. 153). *Homo faber* degrades the world by instrumentalizing it, thus depriving it of any intrinsic value.

Animal laborans and *homo faber* are participants in the *vita activa,* but the only uniquely human activity is that of acting and speaking, or simply, *action.* "A life without speech and without action . . . is literally dead to the world; it has ceased to be a human life because it is no longer lived among men" (1958, p. 176). The condition of action is plurality, and plurality is unique to man. Plurality, as Arendt conceives it, combines the sameness of the species and the diversity of individuals. This plurality is a potential given by the fact of natality, the birth of new human individuals, but it can be realized only through political association. It is in their acting and speaking together that unique individuals emerge out of the sameness and eternal recurrence of the species. And it is only when living together as acting beings in political association that human beings encounter other human beings, that plurality is realized. While in ordinary usage we often distinguish between action and speech, Arendt's category of action combines speaking and doing. In fact, action would be impossible without speech, for "speech is what makes man a political being" (1958, p. 3). Speaking as a persuasive public act is prototypical of action. Thus Arendt distinguishes action both from behavior, which is the predictable and automatic obedience to norms, and from purely instrumental activity, which is merely putting into practice a preconceived plan.

Arendt's distinction between political action and instrumental activity is further developed in her discussion of the identity-revealing quality of action and in her emphasis on the uniqueness and novelty of that which is revealed. "In acting and speaking, men show who they are, reveal actively their unique personal identities and thus make their appearance in the human world, while their physical identities appear without any activity of their own in the unique shape of the body and sound of the voice." This unique identity which is revealed in action is not the objectified essence of the individual, not simply a composite of "his qualities, gifts, talents, and shortcomings" (1958, p. 179). It is the uniqueness of the individual as subject, as acting human being, which can be revealed to others only through action: the acting individual creates his or her own human identity.

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[1] The reader will notice that the account which follows parallels Aristotle in many respects; see *Politics,* Book I.

To act is to introduce into the public realm something which, though intangible, is perfectly real and has consequences of its own. Every action is a new beginning and thus unexpected. This unexpectedness is the appearance of freedom, not freedom of the will or freedom of choice, but "the freedom to call something into being which did not exist before." Thus free action, while not unrelated to motives and goals, is never determined by them and is free only "to the extent that it is able to transcend them" (1968, p. 151). Free action always makes a new beginning, a beginning that is new even to the motives and goals of the actor. To put the same point differently, the meaning of action cannot be comprehended solely by reference to its motives and goals, and beginning has its source in action itself, not in the thinking about or planning of action. Action as beginning is "not bound into a reliable chain of cause and effect . . . it is as though the beginner had abolished the sequence of temporality itself" (1963, p. 207).

One final perspective can be gained on Arendt's contrast between politics and instrumental concerns by looking at her reading of Plato's political philosophy. Plato began the Western tradition of political thought, Arendt argues, by turning away from the life of the *polis* to the life of the philosopher, and this turning finds its best expression in the allegory of the cave which poses a direct challenge to the world order of the *polis.* Plato's fundamental error consisted in the perversion of philosophic truths that are beyond language into standards for the governance of human affairs. The result of this Platonic misconception was the substitution of making for acting in the realm of human affairs. In Plato's philosophy the city is divided into expert and non-expert, ruler and ruled. His authoritative interpretation is based on a set of categories that preclude the conceptualization of the truly political at all. By conceptualizing the political on the analogy of fabrication, Plato destroyed action in the realm of thought.

To summarize briefly the points in Arendt's treatment that concern us here: the historical emergence of politics in the *polis* was based on a separation between the economic affairs of the household and the speaking and acting of citizens in the *agora;* political action is to be sharply distinguished from the instrumental activity of *homo faber;* action as the disclosure of individual identity and the introduction of that which is genuinely novel into the world must transcend all motives and goals of the individual actor; and finally, Plato, developing categories that became authoritative for the Western tradition, destroyed politics in the realm of thought by conceiving it in instrumental or craft terms. Certainly there is sufficient basis here for the contention that Arendt proposes a conception of politics devoid of instrumental concerns and divorced from economics. Let us turn now to the specifics of that contention.

Three Criticisms of Arendt

Three separate critiques of Arendt must be considered here. They advance three different but related criticisms of Arendt's position, and taken together they constitute an interpretation that is widely accepted in the growing secondary literature (see also Schwartz, 1970; O'Sullivan, 1973).

Kirk Thompson's brief but important critique of Arendt occurs in the context of what is in part a very Arendtian argument, the contention that modern constitutional theory is fundamentally deficient because "it does not contain a concept of political action" (1969, p. 655). Arendt's account is inadequate, he contends, because she conceives of politics as nonpurposive; she "maintains that action, to be free, must be free from motive, aim, or goal, and that it cannot be understood in a means-ends framework. Action thus seems to fade away into an existential life-process and lose all clarity of purpose" (1969, p. 659). If Arendt's conception of political action is to have any value, Thompson argues, it must be altered to include a clear specification of the instrumental nature of political action.

In an article that, like Thompson's, combines praise and criticism, Jürgen Habermas focuses his attention on what he takes to be a serious deficiency in Arendt's concept of political action, a deficiency that he links to her rigid separation of praxis from work, labor, and thinking, and her tracing of political power exclusively to praxis:

> This narrowing of the political to the practical permits illuminating contrasts to the presently

> palpable elimination of essentially practical contents from the political process. But for this Arendt pays a certain price: (a) she screens all strategic elements, as force, out of politics; (b) she removes politics from its relations to the economic and social environment in which it is embedded through the administrative system; and (c) she is unable to grasp structural violence (1977, p. 16).

Thus Habermas, like Thompson, focuses on the sharp separation Arendt imposes between politics and instrumental action.

In a very recent piece, Martin Jay continues this critical theme in a manner which further elucidates the connection between the different views on this issue advanced by Thompson and Habermas. Jay places Arendt in the political existentialist tradition of the 1920s and sees her as asserting the primacy of the political realm in contrast to the nineteenth-century tendency to see politics as a function of socioeconomic forces. In her effort to establish the "utmost possible autonomy," Jay continues, Arendt "saw politics not merely as irreducible to socioeconomic forces, but also as unhampered by all normative or instrumental constraints as well. . ." (Jay, 1978, pp. 352–53). Presenting a thoroughly critical reading of Arendt, Jay, unlike either Thompson or Habermas, goes on to suggest that Arendt's conception of politics has totalitarian implications because of the absolute separation she imposes between political and instrumental action, between politics and socioeconomic factors. Continuing but radicalizing the line of criticism developed by Thompson and Habermas, Jay condemns Arendt's vision of politics as pure unconstrained expressive action with no purposes and no criteria for judgment beyond itself.

Motive and Goal: A Closer Look

The criticisms advanced by Thompson, Habermas, and Jay may indeed suggest some important deficiencies in Arendt's political thought generally, as I will discuss later, but all three critics claim to find a fundamental flaw specifically in Arendt's notion of political action. This claim is based on a misreading of Arendt's position. To demonstrate the lack of textual basis for this critique requires taking a closer look at those portions of Arendt's exposition that concern the role of motives and goals in action. We have already seen how action, for Arendt, involves the revelation of the unique identity of the actor and the introduction of something genuinely new into the world; action is always disclosure and beginning. Because action is beginning, it is unpredictable, lying outside any chains of cause and effect. This unpredictability of action makes it appear arbitrary, but only when one attempts to explain its occurrence as one object in a world of objects. This seeming arbitrariness disappears once the true character of action is recognized. As she writes in *On Revolution:*

> What saves the act of beginning from its own arbitrariness is that it carries its own principle within itself, or, to be more precise, that beginning and principle, *principium* and principle, are not only related to each other, but are coeval. The absolute from which the beginning is to derive its own validity and which must save it, as it were, from its inherent arbitrariness is the principle which, together with it, makes its appearance in the world (1963, p. 214).

What otherwise might appear to be totally unrelated to anything, and thus to be a matter of sheer chance, is in fact grounded in an absolute, the principle which manifests itself along with the action. In the human world as opposed to the world of the physicist, one is not forced to choose between the predictability of determined events and the meaninglessness of random occurrences. The free actions of human beings acquire meaning through their inherent relationship with principles. In fact, strictly speaking, action is not merely some occurrence in the physical world, some behavioral event; it *is* its meaning. And the principles which inspire actions and in terms of which they have their meaning must not be mistaken for eternal truths, for principles have their existence only through action. Action and principle are coeval.

Thus, for Arendt, the manifestation of principle, the disclosure of "who," and the revelation of meaning, are three fundamental aspects of action. In order to elucidate these relationships we may begin by taking a closer look at the relation of principle to action:

> Action insofar as it is free is neither under the guidance of the intellect nor under the dictate of the will—although it needs both for the execution of any particular goal—but springs from something altogether different which . . . I shall call a principle. Principles do not operate from within the self as motives do . . . but inspire, as it were, from without; and they are much too general to prescribe goals, although every particular aim can be judged in the light of its principle once the act has been started. For unlike the judgment of the intellect which precedes action, and unlike the command of the will which initiates it, the inspiring principle becomes fully manifest only in the performing act itself. . . . In distinction from its goal, the principle of an action can be repeated time and again, it is inexhaustible, and in distinction

> from its motive, the validity of a principle is universal, it is not bound to any particular person or to any particular group. However, the manifestation of principles comes about only through action, they are manifest in the world as long as the action lasts, but no longer (1968, p. 152).

This passage is an important test for those who interpret Arendt as insisting that action be entirely free from motive and goal. It at once provides the basis for and belies that interpretation. Certainly Arendt says that insofar as action is free, it is determined by neither intellect nor will. But she also writes that action "needs both for the execution of any particular goal." In other words, action does, or more accurately *may*, involve the achievement of specific goals. What is only suggested here, and what will be verified shortly, is that the *meaning* of the action is not prefigured either by the intellect or by the will; rather, it is tied up with the principle manifested in that action.

Thus while Arendt might agree with the contention that some human activity is caused by motives and intentions, she insists that free action is related in a special noncausal manner to principles which may be understood as "inspiring" the action. "Such principles are honor or glory, love of equality, . . . or distinction or excellence, . . . but also fear or distrust or hatred" (1968, p. 152). These principles are universally valid and thus too general to prescribe (or cause) any particular goal (or act); on the other hand, it is in terms of a principle that an individual act can have meaning and be judged. Thus action combines the universality of thought with the particularity of human activity. What the critics fail to understand is that action is a *combination* of the particular, e.g., goals, and the universal, principles of human association. Arendt's point is not that action must have no goals but that it cannot be defined in terms of them. The particular ends of action are always transcended by the general principles which give them significance and meaning. Insofar as a universal principle is manifested in a particular act, it becomes possible to judge that act in terms of what Arendt calls the "greatness" of the act, that is, the greatness of the manifestation of principle. Thus we might say, for example, that a certain action was a great manifestation of the love of equality. In *The Human Condition* Arendt writes:

> Motives and aims, no matter how pure or how grandiose, are never unique; like psychological qualities, they are typical, characteristic of different types of persons. Greatness, therefore, or the specific meaning of each deed, can lie only in the performance itself and neither in its motivation nor its achievement (1958, p. 206).

Because action is free and "neither under the guidance of the intellect nor under the dictate of the will," its *meaning* cannot be explained in terms of motives and aims, but only in terms of the greatness of its manifestation of the absolute which inspires it. In fact, it is the greatness of this manifestation which is "the specific meaning of each deed."

The meaning of action is also interpreted by Arendt to refer to the disclosure of the actor as subject, and it is in this connection that Arendt emphasizes the inescapable intangibility of meaning:

> The manifestation of who the speaker and doer unexchangeably is, though it is plainly visible, retains a curious intangibility that confounds all efforts toward unequivocal verbal expression. The moment we want to say *who* somebody is, our very vocabulary leads us astray into saying *what* he is; we get entangled in a description of qualities he necessarily shares with others like him; we begin to describe a type or a "character" in the old meaning of the word, with the result that his specific uniqueness escapes us. . . . The impossibility, as it were, to solidify in words the living essence of the person as it shows itself in the flux of action and speech, has great bearing upon the whole realm of human affairs, where we exist primarily as acting and speaking beings. It excludes in principle our ever being able to handle these affairs as we handle things whose nature is at our disposal because we can name them. The point is that the manifestation of the "who" comes to pass in the same manner as the notoriously unreliable manifestations of ancient oracles, which, according to Heraclitus, "neither reveal nor hide in words, but give manifest signs" (1958, pp. 181–82, emphasis in original).

Insofar as the meaning of action is the disclosure of the acting subject, that meaning is only revealed in the form of signs and must be comprehended, we might say, indirectly. This is why Arendt argues, with obvious reference to Plato's treatment of politics in the *Republic,* we can never "handle" human affairs. The disclosure of unique identity is something which transcends any "objective" description of the action. To describe the action as an object is necessarily to omit that aspect of it which reveals the actor as subject, as a free being standing up in public for the sake of some principle of human association. It is by virtue of this free staking-of-oneself that the "who" is disclosed. By showing what one stands for, one reveals who one is.

Perhaps the complexities of this account of meaning, principle, and disclosure can be further elucidated using terms Arendt would have been unlikely to have used herself. A particular action may be described most generally as *S* doing *A* in order to achieve *G* for the sake of *P*. Here *S* is the actor revealed as the unique author of this action (*A*), which is also to say that *S* is revealed as an individual taking a public stand by attempting to achieve some goal (*G*) for the sake of a particular political principle (*P*). Several comments are in order here. While all action is likely to involve an element of purposiveness, it is not in virtue of that purposiveness that it is action in Arendt's special sense. In fact, *A* is action properly so-called only insofar as by manifesting *P*, the meaning of *A* transcends the pursuit of *G*. And the principle emerges directly from the action itself. Even when *S* does *A* consciously for the sake of *P*, as may sometimes be the case, the principle which emerges from the act can be considered neither a motive nor a goal of the action, since principles are too general to prescribe specific actions or goals. This is why Arendt emphasizes the courage involved in action, because the actor can never know in advance either *who* he reveals or what or how great a principle will emerge from his action (1958, pp. 36, 186–87). Thus it is worth observing, although Arendt does not make this point, that any action involving a conscious commitment to principle contains an element of faith. To stand up for a principle is always fundamentally risky, since neither the implications of that principle nor its relationship to this particular act can be fully comprehended in advance.

Up to this point our investigation of the role of motives and goals in action has been one-sided because we have discussed action primarily in relation to the individual actor. But action, for Arendt, is not merely individuals doing certain things. It is a specific mode of human being-together. Not only can action take place only where there is political community; it is action itself that constitutes that community. Politics is done in public, in what Arendt calls "the common world":

> The term "public" signifies the world itself, in so far as it is common to all of us and distinguished from our privately owned place in it. This world, however, is not identical with the earth or with nature, as the limited space for the movement of men and the general condition of organic life. It is related, rather, to the human artifact, the fabrication of human hands, as well as to affairs which go on among those who inhabit the man-made world together. To live together in the world means essentially that a world of things is between those who have it in common, as a table is located between those who sit around it; the world, like every in-between, relates and separates men at the same time (1958, p. 52).

The common world, Arendt is saying, exists in two aspects, the world of human artifice and the world of human affairs, and the latter can arise only when men are related by that artifice. The human artifice is composed of those durable products of man the maker, *homo faber*, and it is not to be confused with articles made for consumption by *animal laborans* in the endless biological cycle of laboring and consuming. It is *homo faber* who alone can create that objective aspect of the common world within which human action becomes a possibility. The *natural* condition of man is as a member of the species, a condition with no individuality and utter subjectivity. It is only the creation of *artificial* conditions, artifice and affairs, which permits individuals to stand apart. The world of artifice permits individuals to retain a sense of their own identity through time and to realize their sameness, or equality, with each other. Obviously, this objectivity of the world of things and the resulting sense of sameness are necessary preconditions to action, which relies for its very creation on the possibility of human communication and for its endurance on the capacity for recollection.

If the common world in its aspect of human artifice is a precondition for sameness or equality, then the common world in its aspect of human affairs is the precondition for diversity or uniqueness. Human plurality (combining equality and uniqueness) can only be realized in public, and this realization requires the combined aspects of the common world as artifice and affairs. The precise nature of these relations which permit plurality to be realized in public can be elucidated by a consideration of what Arendt terms the "web of human relationships." This web is simply the common world of human affairs looked at as the context for human action. The passage in which Arendt discusses the web of human relationships is crucial here because in it Arendt explicitly contradicts the interpretation of her advanced by her critics:

> Action and speech go on between men, as they are directed toward them, and they retain their agent-revealing capacity even if their content is exclusively "objective," concerned with the matters of the world of things in which men move, which physically lies between them and out of which arise their specific, objective, worldly interests. These interests constitute, in the word's most literal signifi-

> cance, something which *inter-est,* which lies between people and therefore can relate and bind them together. Most action and speech is concerned with this in-between, which varies with each group of people, so that most words and deeds are *about* some worldly objective reality in addition to being a disclosure of the acting and speaking agent. Since this disclosure of the subject is an integral part of all, even the most "objective" intercourse, the physical, worldly in-between along with its interests is overlaid and, as it were, overgrown with an altogether different in-between which consists of deeds and words and owes its origin exclusively to men's acting and speaking directly *to* one another. This second, subjective in-between is not tangible, since there are no tangible objects into which it could solidify; the process of acting and speaking can leave behind no such results and end products. But for all its intangibility, this in-between is no less real than the world of things we visibly have in common. We call this reality the "web" of human relationships, indicating by the metaphor its somewhat intangible quality (1958, pp. 182–83, emphasis in original).

Here Arendt quite clearly states what her critics interpret her as denying, that most action is concerned with interests. Indeed, that may be its "exclusive" concern! That is, action typically involves the pursuit of particular goals; it is based on certain motives and intentions. But where action is involved, this structure of interests, motives, and goals is "overlaid" with a subjective meaning which, as we have already seen, is constituted when individuals take a public stand for the sake of some principle of human association, thus revealing who they are. Such stands are made in the context of conflict and cooperation concerning objective interests, but in their world-constituting power they transcend those mere interests which they are about (see also Isaak and Hummel, 1975). In other words, there is a distinction within the process of speaking and acting which parallels that between the human artifice and the web of relationships. Most often action and speech are *about* some objective reality (things or interests) at the same time that they *disclose* the uniqueness of the individual actor. The web of relationships produced by this disclosure is "subjective" precisely because it constitutes the realization of men as subjects, as actors.

In this crucial passage Arendt also clarifies the relationship between the two aspects of the common world, the objective and the subjective, artifice and web. She begins by qualifying her statement quoted earlier that the world of things "relates and separates men." It now appears more precise to say that this relating and separating is a result of the "specific, objective, worldly interests" which arise out of this world of things as men live in it. The human experience of the world of things generates objective (recallable and communicable) interests which relate and bind men together. But it also creates the possibility for diversity by *placing* men in an objective world, that is, by giving them a location, a perspective, and this unique perspective is what gives meaning to action. "Being seen and being heard by others derive their significance from the fact that everybody sees and hears from a different position (1958, p. 57). In turn, of course, this possibility of diversity can only be realized in the realm of human affairs. It is because of this relationship between the human artifice and the possibility of diversity that Arendt can say, "The human artifice, . . . unless it is the scene of action and speech, of the web of human affairs and relationships and the stories engendered by them, lacks its ultimate *raison d'être*" (1958, p. 204). The products of *homo faber* find their ultimate justification in their function as a setting for human action.

Arendt's concept of plurality is not merely formal but substantive and normative. As we have already seen, Arendt specified plurality, combining sameness and diversity, as the fundamental condition of human existence corresponding to action. But plurality is not a natural fact, except as a human possibility; plurality can only be realized in action. And most importantly, not all forms of association involve plurality.

When Arendt is attending to the explicitly substantive, as opposed to the merely formal, meaning of "public" as it hinges on her conception of plurality, she often uses the phrase, "the space of appearance," by which she refers to that stage provided for political action whenever a group of people are together as actors:

> The space of appearance comes into being wherever men are together in the manner of speech and action, and therefore predates and precedes all formal constitution of the public realm and the various forms of government, that is, the various forms in which the public realm can be organized. Its peculiarity is that, unlike the spaces which are the work of our hands, it does not survive the actuality of the movement which brought it into being, but disappears not only with the dispersal of men . . . but with the disappearance or arrest of the activities themselves (1958, p. 199).

Arendt identifies power as the public characteristic which maintains the space of appearance.

She is careful to distinguish power from strength, force, and violence:

> Power is what keeps the public realm, the potential space of appearance between acting and speaking men, in existence.... Power is always, as we would say, a power potential and not an unchangeable, measurable, and reliable entity like force or strength. While strength is the natural quality of an individual seen in isolation, power springs up between men when they act together and vanishes the moment they disperse.... What keeps people together after the fleeting moment of action has passed ... and what, at the same time, they keep alive through remaining together is power (1958, pp. 200–01; also see 1972, pp. 142–55).

Power, then, might be called a public capacity or virtue. Like the virtue of the individual as actor, power is only actualized when the body politic is acting, but it is power as the potential of that body to act, so long as it retains its identity, which maintains the space of appearance. Power is the public virtue of a political association; it is the political virtue par excellence.

Power can only be realized in certain settings, however, only when men are together in a particular way. "Power is actualized only where word and deed have not parted company, where words are not empty and deeds not brutal, where words are not used to veil intentions but to disclose realities, and deeds are not used to violate and destroy but to establish relations and create new realities" (1958, p. 200). The power of the body politic will be dissipated by systematic hypocrisy and by any *use* of the space of appearance merely to accomplish goals without assuming the risks inherent in disclosure. To use the political realm as a means to nonpolitical ends will lead to the destruction of that realm:

> This revelatory quality of speech and action comes to the fore where people are *with* others and neither for nor against them—that is, in sheer human togetherness. Although nobody knows whom he reveals when he discloses himself in deed or word, he must be willing to risk the disclosure, and this neither the doer of good works, who must be without self and preserve complete anonymity, nor the criminal, who must hide himself from others, can take upon themselves.... Because of its inherent tendency to disclose the agent together with the act, action needs for its full appearance the shining brightness we once called glory, and which is possible only in the public realm.
>
> Without the disclosure of the agent in the act, action loses its specific character and becomes one form of achievement among others. It is then indeed no less a means to an end than making is a means to produce an object (1958, p. 180, emphasis in original).

Looked at in terms of motives and intentions, political action transcends concern either for or against individuals. Any activity which can be completely explained as an effort to do something for or against another individual is not action but merely achievement, which can be understood in terms of the craft model of action. As we have already seen, only activity which involves a public commitment to principle, regardless of whether that commitment is consciously intended, can reveal the acting subject. Arendt's notion of "sheer human togetherness" might best be thought of as the associational analogue of this commitment to principle. To act for the sake of principle is to be together in the mode of sheer human togetherness; it is to be together as subjects not objects. Action degenerates into achievement "whenever human togetherness is lost."

Arendt develops another perspective on this aspect of action in her analysis of understanding, that mental activity through which we came to make sense of the human world (1953, pp. 377–92). The activity of understanding directs itself toward questions of meaning, meaning as revealed in political action. Thus understanding is in an important sense a political ability, and it appeared historically, Arendt argues, only with the experience of life in the *polis.* As a result of speaking and acting in public, the Greek citizen acquired the ability to experience the common world as others experienced it, and to acquire this ability, Arendt suggests, is precisely what we mean by developing understanding.

In fact, Arendt's political conception of understanding and her notion of "sheer human togetherness" are closely related. The political experience depends on the ability of the citizens to exchange perspectives, to experience the common world as citizens, that is, as actors. Thus I can only understand the other's viewpoint so long as I view him as an actor, as a subject, rather than as an object. It is this characteristic of political association which I believe Arendt has in mind when she says that citizens must be neither for nor against each other. Citizens are in a relationship of "sheer human togetherness" when they experience each other as the creators of meaning through speech and action in the public realm. "Sheer human togetherness" becomes both a condition for and a description of political association, in which men are engaged in acting and understanding. And thus it is only when associated as political equals that the unique human identities of individuals can emerge into the objective world.

Now we can see more clearly the nature of the relationship between the meaning of action and the motives and goals of the actor. Any given instance of action will involve an actor with motives and goals, but insofar as it is political action, its meaning will transcend those motives and goals and can never be comprehended by an account of them. Insofar as it is an action, it is constituted by the revelation of meaning, and thus can only be judged in terms of greatness. This transcendent quality of action depends, as we have seen, on the existence of a political association, which is to say, it requires the realization of plurality. The character of political association is an important key to Arendt's theory of action.

When examining action from the perspective of the individual, we saw that action transcends motive and intention and is inspired, but not determined, by the principle which it manifests. Now we see that action transcends the individual in another sense, in that action can only occur where people are assembled in a state of sheer human togetherness. Strictly speaking, however, we are not concerned here with two types of transcendence but with a single transcendence viewed from two perspectives. The transcendence of motive and intention *is* self-transcendence just as political action *is* a mode of human being-together. The acting *is* the being-together. It is only in the context of an association based on the mutual recognition of each other as subjects that human beings are able to act—to manifest principles, to disclose themselves as political agents, and to reveal the meaning of unique deeds.

One of Arendt's great insights, as revealed in the preceding explication of the relevant texts, is her subtle and powerful conception of the role of motives and goals in political action. Having examined her position in some detail, we now turn to a consideration of the validity of those criticisms advanced by Kirk Thompson, Jürgen Habermas, and Martin Jay.

Three Critiques of Arendt Evaluated

Thompson. As we have already seen, Kirk Thompson argues that Arendt's account of politics is inadequate because it omits purposiveness. He proposes his own understanding of political action, using four characteristics drawn from Arendt with the addition of purposiveness. It can now be shown that Thompson misreads Arendt on the relationship of purposiveness to action and that his proposed redefinition of political action forfeits the insights provided by Arendt.

On the first point, Thompson (1969, p. 659) reads Arendt as maintaining "that action, to be free, must be free from motive, aim, or goal, and that it cannot be understood in a means-ends framework." In fact, Arendt does *not* define action as "free from motive, aim, or goal." She states explicitly, and directly to the contrary, that most action concerns specific purposes and interests. She writes, for example, that action needs the "guidance of the intellect" and the "dictate of the will" for "the execution of any particular *goal*" (1968, p. 152, emphasis added). Her point is not that politics must be free from these concerns but that it transcends and is not determined by them. And because the meaning of action transcends those motives and goals that inform it, action cannot be comprehended "in a means-ends framework." Typically action involves instrumental considerations, but any "action" that was *totally* comprehensible in a means-end framework would be "action" that instrumentalized human relations and thus not political action at all. Political action involves motives and goals but is not determined by them. And in transcending these specific concerns, action transcends purely instrumental categories.

Turning to the second point, it is important to understand how Thompson's reformulation of Arendt's concept of action sacrifices her most valuable insights. An examination of Thompson's five defining characteristics—novelty, indeterminacy, a public quality, consequentiality, and purposiveness—reveals his failure to appreciate Arendt's insight into politics as a particular mode of human being-together. His discussion of the five characteristics focuses almost entirely on the activity of the individual actor. The only significant reference to the political community as opposed to the individual occurs in his discussion of the public quality of action: "To be 'public' an action must be undertaken with reference to the public or political good, rather than to private, socioeconomic interest," and such action "is directed to the public benefit" (Thompson, 1969, p. 658). But the notions of political good and public benefit remain abstractions. They are treated as natural facts rather than as problematic human creations that can arise only where human beings are together as political equals. In fact, the very notion of human freedom, as understood by Arendt, is lost in Thompson's reformulation. For Arendt the emergence of the individual from the life of the species constitutes human freedom, but freedom is not an individual affair. Individuals cannot act on their own. Action requires the cooperation of one's fellows, and that cooperation must be freely given, it cannot be commanded. Human freedom emerges only where individuals stand to each other in sheer human togetherness, that is, where all share a commitment to a mode of being together that recognizes and realizes the capacity for freedom in all individuals. Free individuals emerge on the basis of a collective commitment to that freedom. In discussing the public quality of action, Thompson indicates that action must take place in public but does not show why this is essential. He fails to show how the possibility of freedom is premised on a *particular* mode of being-together.

Because Thompson's reformulation of Arendt sacrifices her critical concept of freedom, it also forfeits any potential for understanding the loss of political freedom. For Thompson two of the five characteristics of action are primary: purposiveness and public quality. He sees politics as action undertaken in public for some public purpose. But thus to define politics in terms of the nature of its purposes makes it impossible to talk about instrumentalizing the public realm. And it is exactly this kind of perversion of politics that concerned Arendt and that she sought to warn us against. Given Thompson's reformulation, such a perversion is simply defined out of existence. In other words, on Thompson's definition, politics could involve a number of individuals, each with an overriding commitment to the realization of his or her own conception of the public interest and with a willingness to manipulate other political actors in whatever ways might be necessary in order to realize those goals. For Arendt, such a state of affairs would not be political because the mode of being-together would be essentially instrumental. And that is why for Arendt politics requires the elevation of principle over purpose. To make purposes—even public purposes—primary, as Thompson does, is to lose the essence of political action: its basis in a particular mode of human association that is founded in a shared commitment to freedom. Arendt's conception provides us with what Thompson's reformulation does not, the conceptual tools required to distinguish among different modes of human association, only one of which is political.

Habermas. The article by Jürgen Habermas on Arendt's concept of power is particularly important here because it combines a well-developed appreciation of certain often-overlooked aspects of Arendt's thought with another statement of the line of criticism advanced by Thompson. Habermas provides valuable insights into Arendt's work by reformulating some of her central ideas in terminology drawn from the traditions of phenomenology, critical theory, and Marxism, and in doing so Habermas also illuminates certain affinities Arendt has with those traditions. He refers, for example, to the phenomenological nature of her method: "She attempts to derive from the structures of unimpaired intersubjectivity the conditions of the public-political realm that must be met if power is to be communicatively engendered or expanded" (1977, p. 20). In fact, Arendt's position might be termed political phenomenology because of the centrality of her claim that, as Habermas puts it, "Communicative action is the medium in which the intersubjectively shared life-world is formed" (1977, p. 8). Habermas also calls attention to Arendt's discussion of sheer human togetherness and the nature of political community, underlining the connections with his own recent work on communication and the ideal speech situation: "Hannah Arendt insists that a public-political realm can produce legitimate power only so long as structures of nondistorted communication find their expression in it" (1977, p. 9; also see Habermas, 1970a, pp. 205–18; 1970b, pp. 360–75). Finally, Habermas points out the very interesting and suggestive relationship between Arendt's concept of power and the Marxian and neo-Marxian notion of praxis (1977, pp. 6, 13).

On the question of the relationship between instrumental concerns and political action in Arendt's work, Habermas provides a formulation that is consistent with my explication above:

> The communicatively produced power of common convictions originates in the fact that those involved are oriented to reaching agreement and not *primarily* to their respective individual successes. It is based on the fact that they do not use language "perlocutionarily," *merely* to instigate other subjects to a desired behavior, but "illocutionarily," that is, for the noncoercive establishment of intersubjective relations (1977, p. 6, emphasis added).

The operative words here are "primarily" and "merely." And one would suppose that this choice of words was motivated by a reading of Arendt similar to the one being advanced here: action may involve individual, private concerns, but they are not "primary." It may involve efforts to shape the behavior of others but is never "merely" manipulation. Notwithstanding this accurate formulation of the relationship, however, Habermas goes on to insist that Arendt sets up a notion of politics devoid of instrumental and strategic concerns:

> I want only to indicate the curious perspective that Hannah Arendt adopts: a state which is relieved of the administrative processing of social problems; a politics which is cleansed of socio-economic issues; an institutionalization of public liberty which is independent of the organization of public wealth; a radical democracy which inhibits its liberating efficacy just at the boundaries where political oppression ceases and social repression begins—this path is unimaginable for any modern society (1977, p. 15).

Notice the dramatic change in language from the earlier passage quoted above. To take but one example, it is no longer that politics must not be *merely* a matter of socioeconomic issues; now it must be "cleansed" of them. But, as I have argued, there is no basis in Arendt's writing for this interpretation. Arendt does not argue that politics must be cleansed of socioeconomic issues; in fact, she says it is typically about them. Her point is that truly political concerns must form the context of purposes in terms of which socioeconomic issues are evaluated rather than socioeconomic purposes determining political issues. A radical democracy must indeed be concerned with eliminating social oppression—not, however, in pursuit of some ultimate goal of economic justice, but rather because social oppression destroys the conditions for political community. Arendt's contention is simply that the strategic concerns must never become ultimate ends; they must always be civilized by submission to the light of political principles.

Ironically, in criticizing Arendt, Habermas himself employs a rigid distinction between, on the one hand, the acquisition and maintenance of political power, involving power struggles and strategic considerations, and, on the other hand, the generation of power understood in terms of Arendt's concept of political action and community. Habermas argues that, according to Arendt, "strategic contests for political power neither call forth nor maintain those institutions in which that power is anchored" (Habermas, 1977, pp. 17–18). But this is clearly not what Arendt says. Indeed, it *may* be true that the activities of a particular contestant for political power are *totally* determined by means-ends calculations which reduce all other persons involved to instruments. However, it is also possible that a contestant for power may perform exactly that sort of principled action that rejuvenates the tradition of political freedom and invigorates the body politic. Arendt's point, quite simply, is that while political action *may* involve the struggle for power, it is political only insofar as it rises above the level of mere struggle and, to whatever degree, rejuvenates and invigorates the life of the political community. Certainly it is true that Arendt's understanding of politics—her emphasis on its transcendence of particular goals and its embeddedness in sheer human togetherness—implies that politics will be particularly fragile in power struggles, especially in revolutionary situations. In such circumstances strategic goals are particularly likely to become all-determining to the exclusion of politics. But such an eventuality is not inevitable; the shared commitment to human freedom can persevere. Arendt recognizes the fragility of politics in the struggle for power, but that does not make her conception irrelevant to strategic concerns; quite the opposite. She provides an insight into the potential corrupting influence of the struggle for power and a vision that may help avoid that corruption.

Jay. Martin Jay's critique of Arendt relies heavily on her alleged dichotomy between instrumental concerns and political action. Jay extends this critique further than does either Thompson or Habermas, however, arguing that her notion of politics as unconstrained expressive action has totalitarian implications. These implications follow, Jay suggests, from Arendt's effort to give politics the "utmost possible autonomy" from socioeconomic factors. In attempting to establish this autonomy, Jay continues, Arendt "saw politics not merely as irreducible to socioeconomic forces, but also as unhampered by all normative or instrumental constraints as well. . . ." Further, "she seemed to conclude that action should be free of even purely political goals (e.g., persuading one's opponents) as well as nonpolitical ones, which is characteristic of the binds into which she sometimes fell" (Jay, 1978, pp. 352–53).

As we have already seen, Arendt did not view politics as *unhampered* by normative or instrumental constraints, but as not *determined* by them. She does indeed insist that the greatness of political acts is not a moral question, but Jay overlooks the normative content of her notion of action itself as revealed in our earlier discussion of principles of action and sheer human togetherness (see also Botstein, 1978, esp. pp. 371, 375, 379). And, of course, action is typically about, and thus constrained (but not determined) by, instrumental concerns. Jay's contention that action must not involve an effort to persuade one's opponents has, so far as I can determine, no basis whatsoever in Arendt's writing. And, as I have argued, her important concept of sheer human togetherness implies just the opposite. Each member of a body of peers must be willing to be persuaded by the speech and action of others. Again, her point is that when persuading one's opponents becomes the overriding goal, political community is destroyed.

Because of his misreading of Arendt, Jay finds her view of politics indefensible and filled with contradictions:

> Both conceptually and historically, her view of politics as a performing art utterly uncorrupted by extraneous considerations is without foundation.... Her frequent insistence on birth, or "natality" as she insisted on calling it, as the prototype of these beginnings ties action to the rhythms of the natural world, which she usually denigrated as the sphere of the *animal laborans*. Likewise, her assertion that politics and utilitarianism are incompatible is undercut by her acknowledgement that the men of the polis did have an implicit goal beyond the sheer joy of political participation: the achievement of worldly immortality through the performance of glorious and memorable deeds. Such an admission, of course, begs an important question, for how can the criteria used to establish what are "glorious and memorable deeds" escape being nonpolitical themselves? (Jay, 1978, p. 363).

As we have already seen, Arendt views politics as transcending "extraneous considerations" and thus providing a context of public commitments in terms of which those considerations can be judged. Such considerations are neither extraneous nor corrupting; they are the stuff of politics. They become corrupting only if they assume the status of overriding goals. Similarly, the tie which natality provides between action and the natural world is no contradiction. Action emerges out of the natural world and transcends it. Human beings are natural beings who transcend their animal nature in political community.

Finally, Jay's allegation that Arendt contradicts herself when she acknowledges worldly immortality as a goal of action raises an interesting point. Arendt does see the desire for worldly immortality as an important motive for political action, but that immortality is a unique sort of goal. It requires both the continued existence of the body politic and action that is great within the context of that tradition. Thus as a goal it is *not* one which uses the public space for ulterior motives but rather one which necessarily submits to the judgment of the political community. Furthermore this "goal" is not a goal in Arendt's strict sense of being a calculable end toward the realization of which specific means can be determined. Greatness, touching the hidden (to the actor) meaning of an act, can never be calculated in advance. Thus worldly immortality and the continued existence of the political community are similar and related "goals": not goals in the narrowly instrumental sense but concerns and commitments that call forth and inspire political action.

The logic of Jay's critique leads him to accuse Arendt of developing an heroic notion of politics that, on her own understanding of totalitarianism, has totalitarian implications. In her work on totalitarianism, Jay argues,

> one of the most sinister characteristics of totalitarian systems, best shown in the Nazi attitude toward the Jews, is their indifference to utilitarian considerations. A politics that is *oblivious* to the means-ends continuum and the consequences of its actions risks descending into the realm of fantasy in which the inexorable logic of an ideology can justify even self-destructive behavior. The "expressive" moment of politics need not be seen as the *absolute negation* of the instrumental (1978, p. 364, emphasis added).

But of course Arendt's notion of politics is neither "oblivious" to consequences nor the "absolute negation of the instrumental." What Jay produces here is a caricature of Arendt's actual position (see also O'Sullivan, 1973, pp. 183–98, and Schwartz, 1970, pp. 144–61).

As was the case with Habermas, Jay's criticism seems to be based on exactly the assumption for which he criticizes Arendt. Jay assumes that politics must either be defined as instrumental action or be reduced to pure expressive action, and then he charges Arendt with self-contradictions because at one point she insists on the essential expressive nature of politics while at another she admits its instrumentality. In fact, her profound insight is that it is the expression of principles of human association in politics that gives meaning to the intentions and goals of the actor, rescuing them from mere instrumentality.

Conclusions and Comments

Hannah Arendt's writing on politics illuminates aspects of political action that have been little explored: politics as the expression of individual identity and political principle, politics as the creation of an intersubjectively shared life-world, politics as the creation of a uniquely human mode of being-together, political community as praxis. She directed much of her attention to the examination and elucidation of these widely ignored qualities of political action and to their public discussion within an intellectual climate largely hostile and blind to them. She was deeply troubled by the course of modern history as well as by such efforts to come to terms with that history as materialist Marxism and utilitarian liberalism. What she saw in these events and intellectual traditions was the loss of humanity. Her writing must be understood as a response to and reaction against that situation. As Leon Botstein has put it, "Her ambitions were to act through speech within historical events, to speak with impact to a broad thinking readership" (1978, p. 369). To understand and appreciate her work requires that it be read in light of these concerns.

But if her emphasis was on these largely ignored characteristics of politics, she displayed no lack of awareness of those other, more widely recognized, aspects of political action: politics as purposive action with motives and goals, politics motivated by socioeconomic interests, politics as the struggle for power. Nor did she try to purify politics by defining it to exclude these elements. What she did was choose language, focus, and emphasis as part of an effort to act against history. Why should she emphasize the instrumental aspect of all politics when her aim was to overcome its instrumentalization and trivialization? Why should she elaborate the strategic aspects of politics when her goal was to recommend politics as an activity transcending the mere struggle for power? And this is not to say that the value of her thought is limited to its illumination of those ignored aspects of politics. As I have argued above, she provides us with a powerful insight into the complex and subtle relationship between politics as instrumental and strategic action and politics as expression and praxis.

Arendt's work does, however, suffer from a deficiency that is related to the line of criticism examined here in the work of Thompson, Habermas, and Jay. This deficiency lies not in Arendt's concept of political action but in her rather one-dimensional treatment of economic activity. Perhaps because of her reliance on the household economics model of the *polis,* she gives insufficient consideration to the great variety of possible modes of economic organization and to their different political implications. In focusing on the negative impact of economic concerns on the political realm, she overlooks the possible positive effects of certain modes of economic organization as well as the potential humanizing of economic relations that could arise out of political association. Just as the motives and goals involved in action must ultimately be evaluated in terms of political principles, so various modes of economic organization must be evaluated in terms of their implications for the political life of the community as Arendt conceived it. The clear implication of Arendt's work for the economic realm, I would argue, is that the realization of political freedom requires movement in the direction of a decentralized and democratic socialism with extensive worker and community control of economic enterprise (see also Knauer, 1976, pp. 303–34). And the starting point for an investigation of the relevance of Arendt's work to these matters is an appreciation of her insight into the relationship between politics as instrumental and strategic activity and politics as expression and praxis.

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Creating the Statutory State: The Implications of a Rule of Law Standard in American Politics

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The rise of the modern state has been accompanied by a decline in the importance of statutory law to the operation of Western democratic governments. Two of the strongest critiques of the modern state and of this decline in statutory law have come from Friedrich Hayek and Theodore Lowi. Each has argued that only the restoration of a rule of law can ensure the continued survival of democratic societies. Their indictments of the modern state suggest a standard by which legislative policy alternatives might be evaluated. This article develops such a standard, adapted specifically for use in American politics, and uses it to analyze legislative behavior in the House of Representatives under four different presidential administrations. The development and application of the rule of law standard provides a critique of American government from the Hayek-Lowi perspective and reveals a basic incompatibility between the imposition of a rule of law and the establishment of a responsible-party government system.

The most striking characteristic of the modern state has been its reliance on bureaucratic and executive discretion. Most analysts and scholars have accepted this discretion as an unavoidable by-product of the increasing complexity of modern society and a corresponding growth in government power and activity. In particular, bureaucratic discretion has been interpreted as a necessary condition both for private participation in government processes and for government management of the economy. As a result, the rise of the modern state has been accompanied by a decline in the importance of statutory law in the operation of Western democracies.

Two of the strongest indictments of the modern state have come from Friedrich Hayek and Theodore Lowi.[1] Each has argued that increasing reliance on bureaucratic discretion and the decreasing importance of statutory law threaten the survival of Western democratic systems. For Hayek, the discretionary administrative state is incompatible with the survival of individual freedom. For Lowi, increasing reliance on bureaucratic discretion has fragmented and decentralized government, thereby alienating state authority from public control. As a result, Lowi argues that the moral status of government power is increasingly ambiguous and that the legitimacy of the modern state is thereby diminished. In both indictments, the concept of a rule of law is central to a critique of the modern state; however, the concept has never been applied in the American context and its practical implications remain largely unexplored.

The purpose of this article is to specify the practical criteria by which we might judge the implications of a rule of law standard in American politics, and to examine—from the voting records of the House of Representatives—the major contemporary sources of support for and opposition to the concept. An operational standard which discriminates between actual policy alternatives and can be applied within the legislative process is useful for at least three reasons. First, since such a standard could be applied to a large number of policies,

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[1] The following discussion is largely based on Friedrich A. Hayek's *Road to Serfdom* (1944) and *Constitution of Liberty* (1960) and on Theodore J. Lowi's *End of Liberalism* (1979). Lowi briefly discusses the relationship between his work and that of Hayek in his preface to the second edition of *End of Liberalism* (1979, p. xiv): "I neither began nor ended as a Hayekist but instead found myself confirming, by process of elimination and discovery, many of his fears about the modern liberal state." For Hayek, the decline of legislative law coincided with the rise of the administrative state. He cites 1920–30 as the period when the decline was most rapid and names Germany as the country in which the administrative state and its supporting ideologies first arose (1960, Ch. 16). Lowi associates the decline of legislative law in the United States with the rise of "interest group liberalism." In his analysis, the rapid growth in legislative delegation coincided with Supreme Court decisions which incrementally disgarded the substance of the 1936 Schechter decision (1979, pp. 96–97).

it might be possible to determine if statutory law tends, as Lowi believes, to strengthen the legitimacy and effectiveness of state authority without diminishing the overall role of the government in the economy or society. Second, a rule of law standard might suggest groups which, with comparatively minor changes in the structure of American politics, would support its revival in the United States. The identification of such groups would increase the apparent feasibility of both Hayek's and Lowi's recommendations for reform. Third, since the standard must discriminate between actual policy alternatives and since, in many cases, one of these alternatives is the way in which the state is currently structured, the development of a practical rule of law standard entails an evaluation of many of the most prominent features of modern government: administrative structure, judicial practice, constitutional law, and legislative review. This evaluation produces a number of unanticipated conclusions, among them a preference for a unitary national government over a mixed federal system and opposition to legislative veto provisions.

This study begins with a brief review of the Hayek and Lowi theses, follows with the development of a rule of law standard which can be applied within the United States, and concludes with an exploratory analysis of legislative behavior.

The Hayek-Lowi Thesis and a Rule of Law

According to Hayek, state activity in the absence of a rule of law destroys individual liberty (Hayek, 1944, 1960).[2] In the absence of a general statutory rule which would limit its discretion, Hayek maintains that regulation of private economic activity by the state must be accompanied by arbitrary and unpredictable distinctions between individual citizens. The resulting dependency on state favor can be used to stifle dissent and limit individual freedom. Socialization, on the other hand, substitutes the discretion of the state for that of individual citizens. Not only does government ownership narrow the range of permissible private activity, it also increases the number of individuals who are dependent on government employment and contractual relationships. Thus, government intervention can destroy individual liberty by both linking individual welfare to the favor of the state and substituting state discretion for that of the citizen.

Hayek also maintains that certain activities can only be undertaken by the state at an extremely high cost in overall social efficiency.[3] Government policies which either socialize an economic sector or provide for discretionary regulation of private activity decrease the efficiency with which resources are allocated within a society because, in effect, they substitute the centralized judgment of the state for that of private individuals. Since the state almost always knows less about the factors which would have entered into the calculations of private individuals within an economic sector, either preemption or discretionary review is inferior to a competitive and unregulated market. Furthermore, by increasing economic uncertainty in sectors where it is introduced, discretionary regulation by the state discourages private activity in the very areas which most require social investment. Since Hayek recognizes that state intervention is both necessary and inevitable, he urges that government activity be restricted to general statutory rules which can, because of their relative predictability and objective application, enter into the decentralized, self-interested calculations of marketplace actors. In that way, social goals which can only be met through state intervention can be reached with a minimal loss of individual freedom and economic efficiency.

Because he believes that his philosophy is incompatible with modern "conservative" movements, Hayek seems to have a program without a party. As a result, his prescriptions seem to require a radical retrenchment in which a rule of law standard becomes a part of the political traditions which support democratic systems generally. The radical character of his position, highlighted by his unwillingness to describe his philosophy in ideological terms, has two consequences. First, it leaves his analytical prescriptions open to serious charges of impracticality; Hayek can identify the disease but the patient, as he admits, is totally unwilling to take the cure. Second, Hayek almost ends up advocating resistance to all contemporary expansions of state authority because they fail to conform to a rule of law.

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[2] The rule of law has been a major topic in legal theory at least since the rise of the English Parliament. Blackstone, for example, argued that a strong, independent legislature was essential to liberty: "In all tyrannical governments, the supreme magistracy, or the right both of making and enforcing the laws, is vested in one or the same man, or one and the same body of men; and wherever these two powers are united together, there can be no public liberty" (1899, p. 134). Dicey, a century later, described the rule of law as providing for the "absolute supremacy of regular law" over "the influence of arbitrary power" (1960, p. 202). In the United States, the fact that congressional delegation has rarely been interpreted as excessive has discouraged contemporary attention to the principle: "The main thing you need to know about delegation to practice law in the federal courts," writes Kenneth Culp Davis (1973, p. 27), "can be said in one simple sentence: Congress may and does lawfully delegate legislative power."

[3] In Hayek's analysis this social inefficiency is the result of two factors: sub-optimal decisions concerning the allocation of resources and the redirection of private investment away from the sectors that are most heavily regulated by the state (1944, pp. 36–42); see also Hayek, 1960, "Postscript").

In contrast, Lowi places greater emphasis on the delegation of legislative authority to the bureaucracy and, as a result, he seldom discusses individual freedom (Lowi, 1979). From his perspective, the fundamental purpose of a rule of law is to force the resolution of political conflict in a representational arena. Because a well-articulated statute expresses the "moral will" of the state in an act closely tied to democratic processes, both the decisiveness and legitimacy of state policies are maximized. From this point of view, the modern state, constructed out of policies which are incompatible with a rule of law, inevitably applies coercion in the pursuit of inconsistent goals, alienates public authority, and thus diminishes its own legitimacy.

Lowi provides several specific methods by which a rule of law, the juridical side of "juridical democracy," can be instituted. He advocates, first, that the Supreme Court return to the rule implied in the 1936 Schechter decision (*Schechter Poultry Company* v. *United States*). If it had not been discarded in subsequent cases, that decision would have limited the amount of legislative authority that a statute could delegate to an executive agency. Thus, Lowi proposes that the Court establish a constitutional rule of law standard. In his second recommendation, Lowi urges that presidents veto legislation which does not conform to a rule of law. Since, as will be shown, presidents encourage and support legislative delegation because they are its primary beneficiaries, this proposal would require a major reorientation in executive priorities and incentives.

Finally, Lowi suggests that administrative decisions be periodically codified, that several major reforms be made in the administrative rule-making process, and that "sunset" limitations on statutory and agency life be enacted. While the sunset proposal can be molded easily into current reform arguments, rule-making reform and codification will often require the close support and cooperation of congressional oversight committees. Given the apparent indifference, if not aversion, exhibited by the committee system to a rule of law standard, these proposals would, like the veto suggestion, require a reorientation in institutional priorities and incentives.

Thus, both Lowi and Hayek have argued that the survival of Western democratic systems depends on the revival of a rule of law. Without such a standard, democratic states will, according to Hayek, inevitably be transformed into socially inefficient and totalitarian institutions; for Lowi, democratic systems operating in the absence of a rule of law alienate authority by dispersing it among private interests and are subsequently unable to make decisive moral choices.

While Lowi's critique of interest-group liberalism is statist in comparison to Hayek's advocacy of a minimal state, neither critique maintains that a rule of law, in and of itself, specifies an overall size for the state. For both authors, the concept applies only to the form of government policy, not the substance.

Rule of Law Criteria

The restoration of a rule of law would require a general increase in statutory articulation: the extent to which statutory language limits bureaucratic discretion by stipulating specific and objective standards, rules, and penalties and provides for responsible agency performance. The five criteria below both combine the Lowi and Hayek positions on a rule of law and describe the basis for a standard relevant to American government (Hayek, 1960, Ch. 14). The first requirement specifies that law should not apply to past conduct. While most types of retrospective legislation are constitutionally prohibited, some forms of economic regulation and tax provisions would fall under this criterion. The second requirement states that statutory language should contain a clear description of the activity to be undertaken by the state. The terms *specific* and *clear* relate directly to the minimization of bureaucratic discretion.

The rule of law statutory criteria are as follows:

1. The general rule contained in a statute must be prospective, not retrospective.

2. Statutory language must be clear, specific, and publicly advertised. The statute must minimize administrative discretion.

3. The general rule contained in a statute must treat individuals equally in classification and enforcement. The objects of state policy should be classes and categories of citizens or behavior rather than specific individuals or isolated situations.

4. Statutes must be drafted by a governmental body (usually the legislature) not involved in direct enforcement or implementation.

5. All administrative decisions must be subject to judicial review for compatibility with statutory specifications.

The third requirement asks that the state rely on general classes and categories in carrying out policy. Legislation by exception (e.g., private bills in the U.S. Congress) or legislation by itemization (e.g., water projects legislation) would both be proscribed. The fourth criterion separates the legislative statement of government policy from implementation and enforcement. If the two functions are combined in one agency, the state can tailor statutes to individual situations. Judicial review, the last requirement, ensures that conformity to statutory law will be enforced by an agency uninvolved in either the creation or implementation of government policy.

The construction of an ideal rule of law standard based on the above criteria would imply the absolute minimization of bureaucratic discretion. Since the very existence of government activity entails at least some delegation of authority, minimizing rule of law infractions under an absolute standard necessarily implies a state in which government activity itself is minimized. Since the intent of the Hayek and Lowi critiques is to reform the statutory basis of the state, and since their position on this issue does not subsume a minimal state philosophy, a standard which degenerated into an argument for a minimalist state would misinterpret the prescriptive emphasis of both authors. Furthermore, it would be extremely difficult to evaluate most government activity with reference to an absolute standard. The application of such a standard would require judgments of the sort: this activity, in and of itself, does or does not conform to a rule of law ideal.

The rejection of an absolute standard has two consequences. First, statutory articulation —conformity to a rule of law standard—must be considered a relative property. Each of two competing policy alternatives must be compared to the other and not to an ideal conception of a rule of law. Second, the application of the standard in specific legislative situations must be restricted to comparisons of policy alternatives in which the substance of state activity is not at issue; the standard does not discriminate between alternatives which involve an alteration in the general level of state activity. As a result, the standard is limited primarily to comparisons of statutory language concerning penalty schedules, legal definitions, the specification of regulatory authority, types of delegated power, and administrative structure.

Rule of law criteria must also be developed for the most important features of American government: federalism, constitutional law, the status of persons to whom authority is delegated, and legislative veto arrangements. The standard must discriminate between government activities administered solely by the federal government and those involving the cooperation of the individual states for several reasons. In a purely federal program, officials throughout the implementing bureaucracy can be held responsible both to their superiors and to Congress; in a cooperative arrangement including the several states, state officials are relatively immune from such control. For this reason a federal-state program, when compared to a purely national alternative, presents severe administrative problems of conformity and consistency across the individual states. Furthermore, federal-state cooperative programs are inherently ambiguous in execution. Where programs which call for the cooperation of state governments are proposed, the individual states must either pass legislation or otherwise accommodate their own activities to those contained in the proposal. Any state can, by refusing to accommodate the new program, either exclude that activity from its territory or invoke a federally sponsored and implemented substitute within that state. Thus, it is inherently unclear from the statute establishing a cooperative federal-state program which policy alternative, if any, will be implemented within any particular state. For these reasons, wherever a legislative choice is to be made between an exclusively federal program and one involving the cooperation of state governments, the federal alternative conforms more closely to a rule of law. As a result, the standard favors a unitary national state over a mixed federal-state alternative.[4]

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[4] Since the standard is being applied only to federal law in this analysis, statutes must be evaluated both in terms of consistency across the nation as a whole and bureaucratic responsibility to Congress. Fifty different federal-state programs may each be well articulated in

Congressional consideration of constitutional amendments presents several unique problems for the standard. Since proposed constitutional amendments have no judicial history, amendments to them—presented during congressional deliberation—are evaluated by the rule of law standard in much the same way as ordinary legislation would be. However, the final passage of a constitutional amendment raises special difficulties. In that legislative situation, the language of the proposed constitutional amendment must be compared not only to similar language in the Constitution, but also to court decisions which have interpreted that language. Since the consideration of court decisions would introduce complications which would be peripheral to legislative application, motions involving the passage of constitutional amendments are not analyzed in this study.

The standard discriminates between the agents to whom legislative authority is delegated in several ways. A statute that delegates authority to a government official conforms more closely to a rule of law than a statute that assigns discretion to a private individual. This distinction between public officials and private individuals parallels the one made in the discussion of federalism and is based on differential responsibility to the legislature. However, a rule of law does not discriminate between the various types of officials within the national government; in terms of formal responsibility to Congress and their ability to evade or inconsistently implement statutory provisions, all federal officials occupy the same position. Finally, a statute which delegates exclusive authority to a single official or agency conforms more closely to a rule of law than a statute which delegates to a second official veto authority over the actions of a first. Where responsibility for an administrative decision is shared, it is thereby dissipated; a well-articulated statute will clearly assign exclusive responsibility for its execution.

Similar principles guide an evaluation of the role of the judiciary in administrative proceedings. For example, a statutory arrangement in which one government agency may file suit against another imposes an additional layer of discretion upon the authority of the primary administrative agency. Because the first agency may freely decide whether or not to sue, the courts become passive participants in the administrative process and are directly involved only when the review agency decides to use them. For that reason, the probable behavior of the courts becomes a discretionary resource which can be used by one part of the government to influence the actions of another part. Because the resulting administrative structure pyramids the exercise of delegated authority, an arrangement of this type is inferior to one in which there is no review agency. However, where the *objects* of government policy (e.g., state governments, corporations, or private citizens) are allowed the opportunity to obtain judicial review of administrative action, the statute conforms more closely to a rule of law than comparable legislation in which judicial review is denied. Under the first arrangement, the review agency, not itself the object of government policy, can influence the decisions of a primary agency in specific situations without necessarily establishing a general rule or restricting the range of administrative discretion. In the second, because the objects of government policy are permitted to challenge administrative implementation, the courts play a constructive role in articulating statutory meaning and discovering legislative intent.

Congressional veto provisions, which allow Congress to vacate administrative action without passing a law, decrease statutory articulation. These arrangements, entailing the exercise of discretion which does not differ in quality from that exercised by an administrative agency, are evaluated according to the same criteria. Finally, sunset limitations, because they indicate the period of time during which public authority is to be exercised for a legislative purpose, increase the articulation of a statute.

Thus, a rule of law standard discriminates between legislative alternatives according to the extent to which they segregate government activities within the federal system, minimize the breadth of delegated authority, increase the articulation of constitutional language, restrict the sharing of administrative responsibility, and allow the opportunity for judicial review of administrative decisions. Common to each of these distinctions is a concern for the dominant role of statutory law in a democratic society. Because a critique of the modern state from a rule of law perspective has such diverse prescriptive implications, statutory articulation is consistent with neither traditional "liberal" nor "conservative" ideologies.[5]

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terms of state law; however, the federal law would be inconsistently enforced across the various states, would lack a clear description of legislative purpose, and would entail only the weakest responsibility to Congress for performance.

[5] Hayek dedicated *The Road to Serfdom* to "The Socialists of All Parties." In the foreword, he adds

An Empirical Application of the Rule of Law Standard

The criteria developed in the preceding section was used to analyze legislative motions in the House of Representatives for the years 1965–66 (Johnson), 1971 (Nixon), 1975 (Ford), and 1977 (Carter). In order to be included in this study, each motion had to be accompanied by a recorded roll call and its adoption had to either increase or decrease the articulation of the bill or statute upon which it acted.

Within Congress, the two most common legislative situations in which a rule of law standard can discriminate between policy alternatives occur when an amendment is offered to a bill and when a bill is passed. In the former case, the language of the amendment was compared to the section of the bill to which the amendment was offered. If the bill were more articulate with the amendment than without it, the amendment was in relative conformity to a rule of law. Upon passage, the entire bill was compared to those parts of existing law that it would add to or change. If that body of existing law were more articulate with the adoption of the bill, the bill itself was in relative conformity to the standard. A motion to recommit with instructions or to instruct conference committees was also included in the analysis if the effect of the motion on the language of a bill was clearly stated and either decreased or increased the articulation of the bill upon which it acted.

The analysis excluded amendments or bills which would have created, dismantled, or altered the level of government activity. Because their adoption would have implied an evaluation of the size of government programs and agencies and, thus, the size of the state in general, these legislative motions fell outside the scope of the standard. For that reason, if one of the policy alternatives altered the amount of money appropriated or authorized for a program, the motion was excluded from analysis even if the remainder of the alternative would have increased the articulation of the bill. Similarly, motions which limited bureaucratic discretion were included only if they did not necessarily reduce the size of the state. A motion which reduced mandated or obligatory state activity (e.g., narrowed eligibility requirements for social security payments) fell outside the scope of the standard. However, a motion which reduced the range of bureaucratic discretion without necessarily reducing state activity (e.g., restricting the application of wage and price standards to firms with more than 20 employees) came within the scope of the standard and was included in the analysis. In this case, bureaucratic inaction would have accomplished the same end as the proposed motion and the limitation on discretion, given the language of the statute, did not necessarily reduce the size of the state.

Support for a Rule of Law was computed for each member of the House by dividing the number of roll calls on which that member supported the articulating position by the total number of roll calls on which the member voted. Ninety-four of the 706 roll calls (13.3 percent) recorded in 1977 were included in the study. In 1975, there were 90 rule of law votes out of a total of 612. In 1971 and 1965–66, there were 37 (320) and 33 (394), respectively.

Subsequent analysis produced three important findings: *congressional support for a rule of law is inversely correlated with partisan control of the presidency; Republicans tend to support a rule of law slightly more, on the whole, than do Democrats; and, finally, both party leaders and committee chairs provide less support for the standard than the remainder of their respective parties.*

Table 1 presents rule of law scores by party for each of the four administrations. As can be seen, members of the party controlling the presidency consistently supported the rule of law standard less than the party out of power. This partisan differential was most pronounced during the Johnson and Carter administrations. With a few significant exceptions (for example, federal-state cooperative arrangements or constitutional amendments), the legislative alternative which conformed to the rule of law standard usually delegated less authority to the executive branch than did the opposing position. For this reason, opposition to a rule of law is roughly equivalent, in most instances, to support for an expansion of executive power at the expense of Congress and existing law. As can be seen in Table 2, when presidents chose between the two positions, they almost always opposed the rule of law alternative. Furthermore, congressional support for a rule of law

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(1974, p. xi): "Conservatism, though a necessary element in any stable society, is not a social program; in its paternalistic, nationalistic, and power-adoring tendencies it is often closer to socialism than true liberalism; and with its traditionalistic, anti-intellectual, and often mystical propensities it will never, except in short periods of disillusionment, appeal to the young and all others who believe that some changes are desirable if this world is to become a better place."

was inversely related to presidential support (see graphs). This relationship held even though presidents, on the average, did not publicly indicate a preference on 76 percent of all classified roll calls. Even when presidential support was controlled, however, support for a rule of law was consistently higher among members of the opposing party.[6]

The typical articulating motion, as classified by the standard, was an amendment offered from the floor by a member of the party that did not control the presidency. Over the four administrations, amendments comprised, on the average, 87 percent of all rule of law motions, and members of the party out of power offered 68 percent of these amendments. During the

Table 1. Rule of Law Scores by Party and Administration (Percent)

Rule of Law Support Scores	Carter, 1977 Dem.	Carter, 1977 Rep.	Ford, 1975 Dem.	Ford, 1975 Rep.	Nixon, 1971 Dem.	Nixon, 1971 Rep.	Johnson, 1965–66 Dem.	Johnson, 1965–66 Rep.
85–89.9							.4	1.5
80–84.9			.4				.7	11.0
75–79.9	.4	.7	.4		.4		3.2	24.3
70–74.9	1.8	17.9	1.1		.4		4.2	33.1
65–69.9	6.7	34.5	14.7		2.8		4.2	19.9
60–64.9	10.5	17.9	29.8	2.1	6.1	.6	4.9	2.9
55–59.9	13.7	11.7	18.6	6.2	15.0	5.3	7.4	4.4
50–54.9	12.6	6.2	15.8	18.6	22.0	9.9	7.8	.7
45–49.9	11.9	4.8	11.2	40.7	23.2	26.3	14.5	.7
40–44.9	14.7	3.4	7.7	24.1	11.8	25.1	14.1	.7
35–39.9	16.8	2.1	.4	8.3	9.3	19.9	16.6	.7
30–34.9	9.5	.7			3.7	8.2	11.3	
25–29.9	1.4				2.8	3.5	7.4	
20–24.9					2.4	1.2	2.1	
15–19.9							1.1	
Percentage of members above 50 percent	45.6	89.0	80.7	26.9	46.7	15.8	32.8	97.8
Median score	48.3	65.0	59.1	47.2	46.9	42.1	42.1	72.1
Total N =	285	145	285	145	246	171	283	136

Source: Constructed from roll-call data provided by the *Congressional Quarterly Almanac* (1965, 1966, 1971, 1975, 1977).

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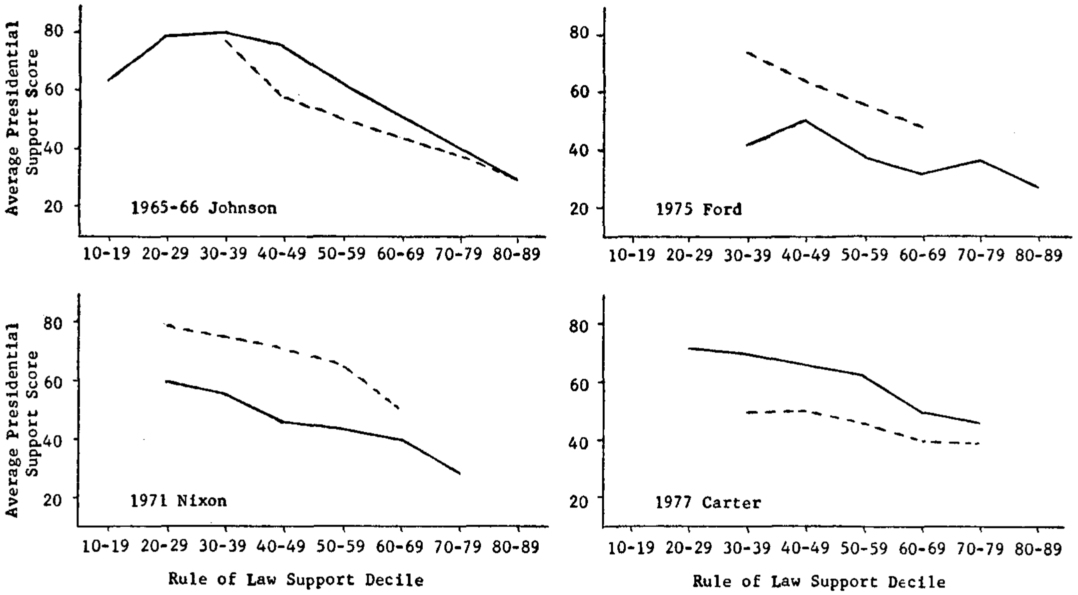
[6] Aage Clausen (1973, Ch. 8) has shown that a change in partisan control of the presidency has usually produced a shift in the policy stance of members of Congress on international involvement. However, a change in partisan control did not produce comparable shifts in domestic policy. While the evidence in the present study does not directly test the proposition, it seems likely that members of Congress pursue similar substantive policy goals regardless of partisan control of the presidency but that these goals take a different statutory form.



Source: Presidential support scores and rule of law support were calculated from roll-call data provided by the *Congressional Quarterly Almanac* (1965, 1966, 1971, 1975, 1977).

Key: Republicans – – – – – –
Democrats ————————

Figure 1. Average Presidential Support Scores by Rule of Law Support Deciles

four periods, 77 percent of those amendments offered by the party out of power articulated statutory language.

Committee chairs, Democrats throughout the period, generally supported rule of law positions less often than did their party (see Table 4). The median support score for chairs was lower than that of the Democratic party as a whole in three of the four periods under investigation. Republican ranking minority members, however, showed a mixed pattern. They gave more support to the standard than their party did in two periods, tied in another, and gave less support in only one. The Democratic members of the Rules Committee provided substantially less support for the standard than the median member of their party during all four periods, while Rules Republicans supported the standard slightly more than the remainder of their party. In general, Democratic committee leaders were significantly more hostile to statutory articulation than were either rank-and-file Democrats or the membership of the House as a whole. Republican committee leaders, on the other hand, tended to provide slightly more support for the standard than did their party and substantially more support than House members generally.

Party leaders throughout all four periods provided more opposition to a rule of law standard than did members of the Rules Committee, committee chairs or ranking minority members, and the median members of their respective parties. For the Democratic leadership (the majority leader, whip, and caucus chairs), this relative hostility to the standard was most pronounced during the Carter and Nixon administrations. Opposition to a rule of law on the part of the Republican leadership (the minority leader, whip, and conference chairs) was greatest during the Ford and Carter periods. For both committee leaders and party hierarchies, this relative hostility to the rule of law standard is somewhat surprising because well-articulated statutory law is the fundamental source of congressional power within the American system.

For individual committees where close reciprocal relationships exist with corresponding executive agencies, broad and unspecific statutes allow a great deal of flexibility. Within the range of discretion thereby granted to the bureaucracy undoubtedly lie countless actions which, though a majority of the oversight committee favors them, would not survive scrutiny on the floor if they were explicitly

Table 2. The Position of the President on Rule of Law Motions

Year	Supported Rule of Law Position	Opposed Rule of Law Position	Took No Position	Total Number of Roll Calls
1965–66, Johnson	2 (6.1)	14 (42.4)	17 (51.5)	33
1971, Nixon	1 (2.7)	5 (13.5)	31 (83.8)	37
1975, Ford	2 (2.2)	12 (13.3)	76 (84.4)	90
1977, Carter	2 (2.1)	10 (10.6)	82 (87.2)	94

Source: The *Congressional Quarterly Almanac* (1965, 1966, 1971, 1975, 1977) provided the president's position on the roll calls analyzed in this table.

Note: Percentages of total roll calls are given in parentheses.

articulated in a statute. Thus, committees may tend to enact broadly worded statutes both in order to provide flexibility in agency-committee interaction and as a method of circumventing potentially hostile majorities on the floor. Floor amendment activity, it appears, is one of the strongest institutional checks on executive agency-committee cooperation. In some respects, then, a strong committee system is basically antithetical to a rule of law.

A similar relationship may explain the opposition to a rule of law exhibited by the leadership of the House. The access to the president that party leaders usually enjoy creates the potential for the same type of interaction that takes place between committees and their bureaucratic counterparts. The president and the party leaders have an opportunity to share and trade influence over those executive decisions which are either too visible or too wide-ranging to remain within the prerogatives of committee-agency spheres of discretion. In both cases, the drafting of specific statutes in committee and their adoption on the floor may represent punitive action against the executive branch and signify a deterioration in the informal relations that usually exist between the executive and legislative branches. The major source of support for a rule of law seems to come from those members of the House who are left out of such relationships: the minority party within Congress, members of the party that does not control the executive, and those members who do not occupy positions of influence in the institution.

This study, because of its exploratory nature, has not investigated the relationship between rule of law support and a number of important variables. For example, partisan control of the presidency, the substance of policy areas, and the quality and extent of committee-agency interaction probably affect statutory articulation independently. A close examination of the influence of each of these factors would help to identify the proximate sources of support for the standard. In addition, the theoretical segregation of a rule of law standard and a minimalist state philosophy would be demonstrated in congressional behavior by a low correlation between opposition to a larger federal role and support for the standard. In this connection, it might be noted that opposi-

Table 3. Characteristics of Floor Amendments to Which the Rule of Law Standard Can Be Applied (Percent)

Rule of Law Amendments:*	1965–66 Johnson	1971 Nixon	1975 Ford	1977 Carter
As a percentage of all rule of law motions:	75.8	91.9	86.9	93.3
Percentage offered by:				
Democrats	20.0	70.6	65.8	41.7
Republicans	80.0	29.4	34.2	58.3
Percentage which support a rule of law:				
Democrats	60.0	83.3	79.2	57.1
Republicans	80.0	60.0	80.0	71.4

Source: Constructed from roll-call data provided by the *Congressional Quarterly Almanac* (1965, 1966, 1971, 1975, 1977).

*Includes motions to instruct a conference committee and to concur in Senate amendments.

tion to the rule of law standard developed here is roughly equivalent to support for "pluralism" or, as Lowi terms it, "interest-group liberalism."[7]

The results of this investigation suggest that a parliamentary-type system in which the president and Congress closely cooperate within a relatively strong party organization may be incompatible with a rule of law, and that, given the Hayek and Lowi critiques, reforms which strengthen such cooperation may ultimately weaken popular support for the democratic state. For this reason, supporters of the rule of law position seem to be at odds with those who advocate a strong, national two-party system. Scholars who have taken this responsible-party government position have "generally argued that parties will never play their proper role in the American system until they become centralized, disciplined, and cohesive national bodies dedicated to formulating, expounding, and implementing policy programs" (Ranney, 1976, p. 42). From the evidence, one must conclude that such programs would often entail large grants of legislative authority and a further

Table 4. Support for Rule of Law by Party Leaders and Senior Committee Members

	Carter 1977	Ford 1975	Nixon 1971	Johnson 1965–66
Democrats				
Majority Leader	35.2	52.5	37.9	34.6
Majority Whip	32.9	53.9	34.3	45.5
Caucus Chair	34.4	64.2	38.1	25.0*
Rules Committee				
Mean	46.1	52.9	45.9	42.4
Median	45.2	51.7	44.1	37.3
Committee Chairs				
Median	41.1	52.9	43.8	45.5
All Democrats				
Median	48.3	59.1	46.9	42.1
Republicans				
Minority Leader	56.2	35.5	40.6	73.1
Minority Whip	55.4	40.7	45.8	71.9
Conference Chair	42.9	50.0	27.6	69.0
Rules Committee				
Mean	63.0	46.8	43.2	76.2
Median	65.9	46.0	48.4	76.0
Committee Ranking Minority Members				
Median	65.5	45.9	42.1	76.0
All Republicans				
Median	65.0	47.2	42.1	72.1

Source: Constructed from roll-call data provided by the *Congressional Quarterly Almanac* (1965, 1966, 1971, 1975, 1977).

*Democratic caucus chair voted on fewer than half of all index roll calls in 1965–66.

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[7] Within each of Lowi's four policy categories (distributive, constituent, regulatory, and redistributive), the degree of policy conformity to a rule of law varies. From Lowi's description and analysis, the policies which are least articulated in statutory language seem to be those concentrated in the distributive and constituent categories. Lowi argues that distributive policies provoke committee-centered politics while constituent policies generate executive-congressional party-based coalitions. Furthermore, both policies tend to produce highly partisan political conflict. Since a revival of a rule of law would probably produce a trend away from policies falling within these two categories and since such a trend would imply a decline in the cohesion and importance of national party organizations, Lowi's analysis is largely in agreement with the argument in the text.

weakening of the role of statutory law in the American political system.

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Can Government Regulate Safety? The Coal Mine Example

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With the 1970 passage of the Occupational Safety and Health Act (OSHA), federal regulation reached the American workplace. Given the newness of the legislation, any firm conclusion on its effectiveness seems premature. However, there is ample evidence on federal safety regulation of a specific workplace: the coal mine. The federal government has been directly involved in coal mining safety for over 35 years, operating under three major pieces of legislation, enacted in 1941, 1952, and 1969. Opposing opinions regarding the effect of this legislation can be grouped into three categories: radical, reactionary, and reformer. A multiple interrupted time-series analysis indicates that, in fact, the 1941 and 1969 regulations significantly reduced the fatality rate in coal mining. Certain conditions seem related to the effectiveness of this safety legislation: birth order, provisions, enforcement, target population, and goals. The first two conditions would appear to work for the success of the OSHA, the latter three conditions to work against it.

Skepticism over the effectiveness of government intervention is spreading. In retrospect, it is clear that the heralded social programs of the 1960s fell far short of their promise. Currently, there are questions about the utility of the government's attempts to manipulate the economy in order to fight inflation, provide energy, or maintain competition. Of course, such doubts do not necessarily stem government involvement. On the contrary, the identification of new problems seems to lead inevitably to further federal action. One of these new areas for federal concern is work safety which, until the 1970 passage of the Occupational Safety and Health Act (OSHA), was the province of state regulation.

The Occupational Safety and Health Act was born of the liberal impulse for reform. The discovery of an intolerable situation, e.g., the hazardous working environment in a wide variety of jobs, generated a demand for remedial legislation (Special Task Force Report, 1973; Stellman and Daum, 1973; Wallick, 1972). Unfortunately, as policy research and contemporary events suggest, legislative intent is not always fulfilled by passing laws and appropriating money. Therefore, we should seriously consider whether, despite its bold design, the OSHA has actually managed to reduce occupational accidents. In this regard, there are strong reasons for supposing the OSHA is another instance of the failure of liberal reform. The very magnitude of the problem argues against its success. How can government effectively police five million workplaces, each a potential source of occupational accidents? Even the very term, "accidents," implies events beyond the control of government, or any other human institution. Perhaps, then, one might argue that the fundamental cause of these accidents lies with individual workers who make mistakes, either out of carelessness or ignorance. In this case, a preferable approach would be education of workers about on-the-job hazards, leading them to demand higher wages, a demand which management tends to avoid by implementing safety measures. Any direct government intervention in this free-market process, according to this argument, would only result in a sub-optimal level of safety.

These considerations provide a prelude to the empirical query, "Has the OSHA made work less hazardous?" Lamentably, we cannot provide a direct answer to this research question. Leaving aside the obvious data-gathering problems, any answer would be premature. The OSHA has existed only since 1970, and it seems hasty to assert anything definite about its effect (for an attempt, see Nichols and Zeckhauser, 1977). Nevertheless, a current assessment of federal safety legislation need not rely solely on speculation. There is one workplace where the federal government has been involved in safety regulation for many years: the coal mine. On this subject, data are plentiful, although their systematic study has been slighted. Further, the origins and intent of the coal mining safety legislation seem typical of much federal regulatory legislation, the OSHA in particular. An examination of the impact of this legislation

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We would like to thank several anonymous reviewers for their helpful suggestions.

should shed some light on the effectiveness of government safety legislation.

There are three major pieces of federal coal mine safety legislation. Below, we first discuss briefly the possible effects of this legislation. Then we evaluate its impact quantitatively through multiple interrupted time-series analysis, a neglected policy assessment tool which seems widely applicable. Next, from this evaluation, we consider the conditions under which safety regulations are effective. Finally, we draw out the implications of the findings with regard to the effect of the OSHA.

The Background of Coal Mine Safety Legislation

In perhaps no other regulatory context is the clash of public and private purposes clearer than that of coal mining safety. In fact, some critics reduce this contest to "safety versus profit." Accepting this dichotomy, Edward Greenberg (1977, p. 13) actually argues that the struggle for coal mining safety captures "the essence of American political life." How successful has the federal quest for safety in the coal mines been? Compared to other regulatory agencies, few have less ambiguous and more commanding measures of success than the Bureau of Mines (later the Mining Enforcement and Safety Administration)—this measure is the number of miners killed or injured.[1] Historically, no other occupation has been so dangerous as mining coal. Since 1900, over 100,000 miners have met death on the job. To this figure could be added the hundreds of thousands of miners who were permanently injured or died a "slow death" from "black lung."

These extremely hazardous working conditions brought early federal attention, beginning with the formation of the Bureau of Mines in 1910. Eventually, in 1941, 1952, and 1969, Congress passed major legislation designed to improve coal mining safety. These pieces of legislation are products of spectacular mining disasters (Bethell, 1972, pp. 77–80; Coleman, 1943, p. 281; Graebner, 1976, pp. 1–2; McAteer, 1973, p. 213; Wieck, 1942, pp. 6, 116). Following a pattern common to other regulatory enactments, public outrage culminated in the passage of laws to prevent recurrence of such unwanted events. Referring to the 1941 Mine Inspection Act, Wieck laments (1942, p. 131), "Dead miners have always been the most powerful influence in securing passage of mining legislation." Ushering in this 1941 legislation were the deaths of 91 miners at Bartley, West Virginia on January 10, 1940 and of 73 miners at Neffs, Ohio on March 16, 1940 (Wieck, 1942, p. 6). A series of explosions in 1951, highlighted by the West Frankfort, Illinois blast killing 119, led to the introduction of various bills and the eventual acceptance of the 1952 Federal Coal Mine Safety Act (Bethell, 1942, p. 79). The latest set of regulations, that of the Federal Coal Mine Health and Safety Act of 1969, came about principally in response to the 1968 explosion in Farmington, West Virginia of the No. 9 mine of the Consolidation Coal Company, which left 78 dead (Bethell, 1972, pp. xii–xiii, 80).

What has been the impact of this regulatory legislation? The responses to this specific question mirror the broader controversy over the effectiveness of government safety regulation. The participants in this particular debate may be grouped, albeit roughly, into three categories: radicals, reactionaries, and reformers.

To the radicals, the coal mine operators' need for profit overrides any concern they may have for miner safety (Caudill, 1977, p. 493; Graebner, 1976, pp. 3–10; Greenberg, 1977, pp. 10–13; McAteer, 1973, pp. x–36; Nyden, 1970, p. 200; Vecsey, 1974, p. 70). As one miner expressed it, the operator "talks safety in the bath house and 'Cut Coal' underground" (Seltzer, 1974, p. 17). Safety is mostly just talked about, because it costs money. To avoid these costs, coal mine owners and operators lobby vigorously to defeat safety legislation. Failing that, according to the radical critics, they "capture" the regulatory agencies, the key legislators, even the miners' union. Bemoaning the domination of the Bureau of Mines by the coal industry, David Brooks, former chief economist for the bureau, commented, "The Bureau of Mines still doesn't know it's a regulatory agency" (Bethell, 1972, p. 96). After the Farmington disaster of 1968, only one West Virginia member of Congress, Ken Hechler, bucked local coal interests and took a strong stand for forceful safety legislation (McAteer, 1973, pp. 181–84). Tony Boyle, president of the United Mine Workers of America at the time of the Farmington explosion, absolved Consolidation Coal of responsibility, explaining that "as long as we mine coal, there is always this inherent danger.... This happens to be one of the better companies" (Greenberg, 1977, p. 6). In sum, the radicals believe that the negative influence of the coal industry guarantees that any regulatory activity will be largely symbolic, salving the public conscience until the next big mine disaster. This disaster, which inevitably erupts, reinforces the radical view that federal mine safety legislation has had no effect.

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[1] These regulatory responsibilities came under the Mining Enforcement and Safety Administration (MESA), separate from the Bureau of Mines but still within the Interior Department, in 1974. Currently, the relevant agency is called the Mine Safety and Health Administration, located in the Department of Labor.

Reactionaries share with radicals the belief that these regulations will not make the mines safer, but their reasons are very different. In the words of George Judy of the Bituminous Coal Operators Association, "I don't think you can legislate safety" (Bethell, 1972, p. 81). From this perspective, laws cannot significantly affect mine safety because they fail to touch on the two principal causes of mining accidents: the inherent dangers in the activity of mining coal and the carelessness of the miners (Bethell, 1972, pp. 14, 81; Graebner, 1976, pp. 94–95, 162; McAteer, 1973, pp. 13–16, 28–31, 231). Summarizing state inspectors' reports in the Progressive era, Graebner (1976, p. 95) illustrates the second point: "Everywhere . . . [miners] rode illegally on mine cars, carried too much powder into the mines, used the wrong oils in their lamps, failed to timber properly their working places, and were careless in handling explosives." Recently, Ralph E. Bailey, chairman of Consolidation Coal Company, the industry's largest employer, asserted that such carelessness, lack of motivation, and human error remain the real causes of accidents in the mines (*Business Week*, 1977, p. 79).

Reformers are more optimistic about the effects of legislation than either radicals or reactionaries. They come from that broad sector of liberal opinion which believes it desirable and possible to enact laws reducing dangers in the mines. Admittedly, the competitive market pushes coal operators to pursue profit over safety. Therefore, if safety is wanted, according to the reformers, the government must impose regulations to induce companies to do what would not normally be in their economic interest (Anderson et al., 1978, p. 349; Hair, 1968, p. 560). And, for the true reformer, this regulatory legislation is more than symbolic. Instead, it is guided by what has been referred to as the British theory of "rigid laws and law enforcement" for coal mine safety (Wieck, 1942, p. 106).

These perspectives—radical, reactionary, reformer—provide an orientation to the coal mine safety issue. It is now appropriate to move to a statistical model which allows us to assess the actual effects of the legislation.

The Effects of Coal Mine Safety Legislation

To evaluate the impact of coal mine safety legislation, we employ *multiple* interrupted time-series (MITS) analysis, a procedure which policy studies have neglected. Application of an interrupted time-series logic to policy evaluation has been advocated for some time (Campbell, 1969; Campbell and Stanley, 1966). However, methodological difficulties, mostly involving the modeling of the interruption and of the error process, retarded its statistical use. These problems now seem largely overcome (Box and Tiao, 1975, Campbell and Cook, 1979; Hibbs, 1977). This methodological resolution has coincided with the appearance of a number of substantive examples of simple interrupted time series (SITS) involving assessment of a single policy intervention (see Albritton, 1979; Lewis-Beck, 1979; McCrone and Hardy, 1978). Thus SITS analysis is a rather well-established research tool among practicing policy evaluators. But interest in MITS has remained essentially in the methodological domain (Box and Tiao, 1975; Hibbs, 1977). This article counters such emphasis by providing a substantive policy investigation which employs the MITS design. We hope that this example will heighten the reader's appreciation of the utility of the design for research in other policy areas. Whenever one wishes to assess the impact of repeated policy changes over time, MITS analysis seems the preferred strategy. A number of areas of federal policy, e.g., housing, antitrust legislation, labor-management relations, would appear to lend themselves to this approach.

This introduction opens the way for detailed consideration of the effects of coal mine safety legislation. Annual figures on a measure of coal mining injuries, Y_t, can be plotted over an extended number of years. The research issue is whether the observations in this time series have been affected by the safety legislation. Take the Mine Inspection Act of 1941 as an example. Informally, to assess the effect of this act, we can simply look at the shift in injuries after 1941. Formally, we worry about whether the observed change is statistically significant, thereby reducing the likelihood of chance differences. Correct estimation of the following regression equation would permit statistical evaluation of this simple interrupted time-series design:

$$Y_t = b_0 + b_1 X_{1t} + b_2 X_{2t} + b_3 X_{3t} + e_t, \qquad (1)$$

where Y_t = annual observations on coal mining injuries; X_{1t} = a counter for years, from 1 to N, the number of observations; X_{2t} = a dichotomous variable scored 0 for observations before 1942, and 1 for 1942 and after; X_{3t} = a counter of years, scored 0 for observations before 1942, and 1,2,3. . . for 1942 and after; b_0, b_1, b_2, b_3 = parameters to be estimated, e_t = error.

The parameters b_0 and b_1 indicate, respectively, the level and slope of the time-series prior to implementation of the 1941 regulations. To evaluate whether b_0 and b_1 were altered by these new federal regulations, we must examine b_2 and b_3. If the estimate for b_2 is not significantly different from zero, then we infer the legislation had no effect on the level of the time series. Similarly, if the estimate for b_3 does not differ significantly from zero, the inference is that the legislation had no effect on the slope of the time series.

Of course, Equation (1) is only expository, for it ignores the impact on injuries of the 1952 and 1969 safety legislation. Introducing these interventions involves a straightforward extension of the preceding logic, and yields a multiple interrupted time-series design represented by the ensuing regression equation:

$$\begin{aligned} Y_t = b_0 + b_1 X_{1t} + b_2 X_{2t} + b_3 X_{3t} + b_4 X_{4t} \\ + b_5 X_{5t} + b_6 X_{6t} + b_7 X_{7t} + e_t, \qquad (2) \end{aligned}$$

where Y_t, X_{1t}, X_{2t}, X_{3t} are defined as with Equation (1); X_{4t} = a dichotomous variable scored 0 for observations before 1953, and 1 for 1953 and after; X_{5t} = a counter for years, scored 0 for observations before 1953, and 1,2,3. . . for 1953 and after; X_{6t} = a dichotomous variable scored 0 for observations before 1970, and 1 for 1970 and after; X_{7t} = a counter for years, scored 0 for observations before 1970, and 1,2,3. . . for 1970 and after. Hence, b_4 and b_5 capture the short- and long-run impacts of the 1952 legislation, b_6 and b_7 those of the 1969 legislation. Through estimation of this model, then, we are able to determine the probable effects of the various pieces of federal safety legislation on coal mine safety.

We have now developed measures for our independent variables, the federal safety regulations, but our dependent variable, coal mining injuries, has been left undefined. While coal mining accidents can be counted a number of ways, we have selected "fatalities per million man-hours" worked during the year, F_t, as our central measure.[2] Several advantages accompany this measure. First, it focuses attention on the most serious type of injury. Second, it also serves as a more general indicator of mining injuries, correlating .81 with "non-fatal injuries per million man-hours." Third, it is standardized, varying neither with the annual number of strike days (as the annual "fatalities per thousand employed" would) nor the size of the work force (as "number of fatalities" would). Fourth, of all available safety measures it is the most relevant for miners, informing them whether their chances of being killed on the job are increasing or decreasing. For this variable, in conjunction with the other variables eventually to be considered below, annual data back to 1932 are obtainable, providing a sample adequate to test the MITS model of Equation (2).[3] Estimating that model with ordinary least squares (OLS) yields:

$$\begin{aligned} F_t = \underset{(18.7)}{1.493} - \underset{(-.1)}{.001X_{1t}} - \underset{(-.9)}{.096X_{2t}} - \underset{(-2.6)}{.045X_{3t}} \\ + \underset{(1.7)}{.151X_{4t}} + \underset{(3.7)}{.047X_{5t}} - \underset{(-1.1)}{.120X_{6t}} \\ - \underset{(-4.0)}{.091X_{7t}} + e_t; \qquad (3) \end{aligned}$$

$$R^2 = .88 \qquad N = 45 \qquad D\text{-}W = 2.3,$$

where the values below the parameter estimates = the *t*-ratios; R^2 = the coefficient of multiple determination; N = the number of observations (1932–1976); D-W = the Durbin-Watson statistic.[4]

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[2] For continuity, we keep the Bureau of Mines description, "million *man*-hours," although since 1973, more than 2,000 women have begun working in the mines (Joffe, 1979, p. 23).

[3] The data for annual fatalities per million man-hours (F_t), nonfatal injuries per million man-hours (N_t), fatalities per thousand employed (E_t), percentage of mines producing less than 10,000 tons of coal (S_t), tons of output per man-hour (T_t), and percentage of miners working underground (U_t), were gathered from volumes of the *Minerals Yearbook*, the Bureau of Mines, Department of the Interior. The figures on annual appropriations for health and safety to the Bureau of Mines (later MESA), B_t, are from the relevant volumes of the *Budget of the United States Government*. For all these variables, observations were available from 1932–1976, except for S_t and U_t, with observations from 1932 to 1975.

[4] For ordinary least squares (OLS) to yield desirable estimators, one necessary assumption is that the error terms are not autocorrelated. A Box-Jenkins (1970) analysis of the autocorrelation and partial autocorrelation functions of the residuals from Equation (3)

The scores on the fatality index range from 1.7 in 1932 to .4 in 1976, indicating that the risk of fatal injury from coal mining is less than one-fourth what it was 45 years ago. How much did federal mine safety legislation contribute to this reduction in the death rate? The R^2 of .88 demonstrates that the ensemble of safety legislation variables accounts for the downward trend in deaths rather well. Inspection of the parameter estimates offers more detail. The short-run changes (1941–42, 1952–53, 1969–70) are statistically insignificant at the .05 level, two-tail (none of the t-ratios of b_2, b_4, b_6 exceed $|2|$). This absence of a significant short-run influence is unsurprising, for one would not expect the safety regulations to be fully operative so soon after passage of the legislation. Rather, if there is a deterrent effect, it should emerge over the span of years, as it indeed appears to.

Proper combination of the Equation (3) parameter estimates provides a simple regression fit within each time period, and these are sketched over the observations plotted in Figure 1. One sees that in the 1930s, without the impetus of federal regulation, the risk of a mortal accident in the coal mines was high, and exhibited no downward drift (regard b_1). However, after the Mine Inspection Act of 1941, the death rate steadily moved downward. This improvement in mine safety is quantified by b_3, which is statistically significant. Again, subsequent to the Federal Coal Mine Health and Safety Act of 1969, we witness a significant slide in the fatality rate (see b_7). Substantively, the gain in safety during this period is far from inconsequential. To illustrate, a coal miner in 1964 was about twice as likely to be killed on the job as a coal miner in 1974 ($\hat{F}_{64}/\hat{F}_{74} = 2.1$).

The 1941 and 1969 federal safety legislation appear to have diminished significantly the risk of receiving a fatal injury while mining coal. However, we have thus far ignored the anomalous results of the 1952 legislation. This set of

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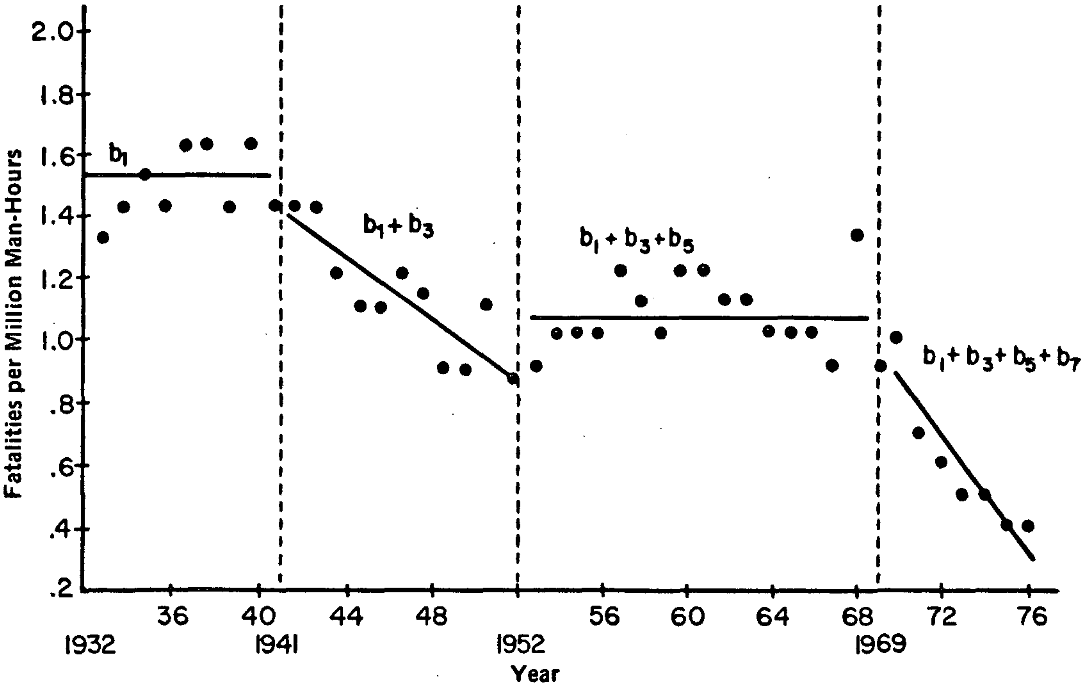
reveals no significant autocorrelation; i.e., an essentially random pattern is observed. More precisely, there is neither a significant first-order autoregressive process (see also the Durbin-Watson statistic), nor any other detectable systematic error process.



Source: Minerals Yearbook, Bureau of Mines, U.S. Department of the Interior.

Figure 1. Multiple Interrupted Time-Series Analysis of the Coal Mining Fatality Rate (1932–1976)

regulations did *not* increase mine safety. Instead, it apparently arrested the downward trend in the death rate stimulated by the 1941 legislation ($b_3 + b_5 \simeq 0$). The stabilization of accident conditions in this epoch is easily seen in Figure 1, where the points trace a flat line from 1952 to 1969. Why did the 1941 and 1969 laws "work," while this 1952 law did not? We pursue this question in detail below. First, though, we must strengthen confidence in the findings of Equation (3). While these results convince us, the skeptic might find them somewhat remote. After all, the independent variables in the equation simply count years, not "real" legislative action. If these time counters are actually capturing the course of legislative impact on coal mine safety, then the Equation (3) findings should survive tests of spuriousness.

Suppose other variables influencing coal mining fatalities are correlated with the MITS independent variables and are actually responsible for the findings. Then, if these "third variables" were controlled for, the apparent regulatory effect would disappear. Of the several variables, besides federal regulation, which might be expected to influence coal mining injuries, we managed to assemble satisfactory measures on three: technology, mine size, type of mining. Technological advances in coal mining involve the introduction of machines to do work formerly done by hand. Such mechanization has frequently been held responsible for heightening the risk of death in the mines (Coleman, 1943, p. 281; McAteer, 1973, pp. 16–17, 198; Wieck, 1942, pp. 79–103). For example, Wieck charges that it increases the amount of explosive gas, coal dust, electricity, fire hazard, and total work time, thus reducing mine safety (1942, pp. 98–100). There is some evidence, however, that technology can make mining less risky (Andrews and Christenson, 1974, pp. 169–73; Hair, 1968, pp. 550–54). Regarding measures of the technology variable, Andrews and Christenson (1974, p. 367) argue on conceptual grounds for "tons of output per man-hour," T_t, which is the indicator we shall employ. Many smaller mines make their appearance during boom periods, only to disappear when demand slackens. Seltzer (1947, p. 9) well describes the historic pattern: "When times were good, a half-dozen strong-backed men with dynamite, picks, shovels, some lengths of mine track, and a couple of ponies could punch a mine into the side of a hill." These and other small mines have been regarded as less safe by some, more safe by others (Andrews and Christenson, 1974, pp. 369–73; Hair, 1968, pp. 554, 558; McAteer, 1973, pp. 237–38). The extent of small mining operations, S_t, will be indicated by their annual percentage in the mine population (technically, this means the percentage annually classified as producing less than 10,000 tons of coal).

There are two general categories of mining: underground mining (or deep mining), and surface mining, which consists of strip and auger mining. While surface mining is dangerous, it is less so than deep mining (National Safety Council, 1977, p. 35). In recent years, the portion of total United States coal production mined underground has decreased (Dials and Moore, 1974, pp. 19–20). The reduced production costs of strip mining, the more limited unionization of strip miners, the low sulfur content of western strip coal, have been offered as reasons for moving away from underground mining (Nyden, 1970, p. 197; Seltzer, 1974, pp. 19–20; Williams et al., 1978, p. 35). This implies the significant safety effect we found for the 1969 legislation is spurious, attributable in fact to the decline in hazardous deep mining which occurred in the post-1969 period. To check this possibility, we introduce the variable, U_t, annual percentage of miners working underground, as a control.

Our original Equation (3) uncovered significant, long-run effects for federal safety legislation (see b_3, b_5, b_7). Here we have cast doubt on these results, speculating they are caused by the "third variables," T_t, S_t, U_t. Below, we enter these variables into the MITS model indicated by the Equation (3) findings, in order to investigate this prospect (the Durbin-Watson statistic from OLS estimation suggested an autocorrelation problem; therefore, the parameters were estimated with the Cochrane-Orcutt procedure; see Kmenta, 1971, pp. 287–89):

$$\begin{aligned} F_t = \underset{(.9)}{.322} + \underset{(1.5)}{.014X_{1t}} - \underset{(-4.8)}{.057X_{3t}} + \underset{(4.1)}{.068X_{5t}} \\ - \underset{(-3.1)}{.124X_{7t}} - \underset{(-.6)}{.131T_t} + \underset{(2.0)}{.006S_t} \\ + \underset{(1.7)}{.009U_t} + e_t; \end{aligned} \qquad (4)$$

$$R^2 = .90 \qquad N = 43 \qquad \text{D-W} = 2.1.$$

Equation (4) provides a more satisfactory specification of the determinants of the coal mine fatality rate. Although variations in mining technology do not appear important, the presence of small mines and the degree of deep mining do contribute significantly to an explanation of mine safety. Smaller mines seem riskier, as do underground mines. These variables round out our understanding of what brings death to the miner, but they do not substitute for the regulatory influences described earlier. In fact, according to Equation (4), the effects of the federal safety legislation are not at all spurious. Instead, we continue to observe the pattern of significant legislative intervention noted in Equation (3), with effects fully as robust (compare b_3, b_5, b_7 from both equations).

The Conditions for Effective Safety Legislation

Thus far, we have not explained why these coal mining safety laws have worked as they do. When we examine the content of the laws, and the context of their passage and implementation, suggestive associations are discovered. These relationships point to some necessary ingredients for effective coal mine safety legislation, as well as for safety legislation generally. Below, we focus on the conditions of enforcement, provisions and position in a sequence of legislation (referred to here as "birth order"). In the conclusion we find it appropriate also to consider the factors of target population and goals.

1941: The "First" Law. Under the Mine Inspection Act of 1941, federal mine inspectors were given the right to enter the mines, where they could carry out inspections, investigate accidents, and make safety recommendations. The beneficial effect of this law, which the MITS analysis detects, is perhaps surprising, for the safety recommendations were *not* mandatory. Why, then, did it bring about a significant reduction in the coal mining fatality rate? The law appears to have been effective because it was the *first* law introduced into a lawless situation. At the national level, the rules for mining safety were suddenly changed from none to some. Until then, safety conditions were determined by the operators, who had neutralized the influence of the state regulatory agencies. In this highly competitive industry plagued by narrow profit margins, safety was routinely sacrificed to production and profit. The arrival of the federal inspectors, who had never before been permitted to enter the mines, unsettled operators, many of whom branded the law as "sheer Communism" (Bethell, 1972, p. 78). Still, the presence of federal inspectors was bound to produce safety improvements, even if grudgingly instituted. Workplace safety had been so neglected that a number of improvements could be made simply and cheaply. Surely, in response to or in preparation for the federal inspector's visit, some operators would be moved to narrow the spacing of the roof bolts, replace a fallen support, remove piles of explosive dust, check the machinery wiring, or put a protective canopy over the mine entrance.

Further, more subtle changes were hoped for as a result of the law. Edward Wieck (1942, p. 139), in his otherwise critical review, expresses these hopes well: "Federal inspection of mines will put the Bureau more closely in touch with the industry and bring about a wider acceptance of its safety standards and procedures, particularly by state mining departments, and probably, in the beginning, to a lesser extent by management." That is, the state agencies would be encouraged to apply the federal standards, and the operators would be made to feel a part of the "community of concern" over safety. The latter seems especially important, for it suggests the owners might be socialized, to some extent, to the more public role of promoter and protector of safety.

The 1941 law, then, derived considerable influence from its "birth order." An implication is that the beneficial effects of this "first" law would be short-lived, in the absence of strong provisions which allow its impact to be sustained. That is, once the operators have carried out the low-cost safety improvements and reached a new psychological equilibrium regarding their role in assuring miners' safety, additional safety effort and, hence, further declines in fatalities would be unlikely. If we examine Figure 1, this is in fact the pattern we observe. The scatter of points in the late 1940s and early 1950s indicates a stabilization of the fatality rate. Safety conditions in the mines were entering a plateau when the 1952 legislation was passed. As the MITS analysis demonstrates, this 1952 legislation was too weak to push the fatality rate below that plateau.

1952: The "Symbolic" Law. By 1952, the energies of the 1941 legislation were spent. A new law was needed to restore the downward momentum in the death rate initiated by the earlier legislation. However, this new law had to have rather firm and comprehensive provisions, for it could not benefit from being first, as the 1941 law had. The federal government had now established itself as a presence in the coal fields, and operators had made the necessary adjustments. Tangible changes had to be forthcoming in order to continue driving down fatalities. Unfortunately, the final provisions of the 1952 Federal Coal Mine Safety Act allowed no more than maintenance of the safety level prevailing toward the end of the period under the 1941 law (see the flat line, 1952–69, in Figure 1).

In examining the failure of the 1952 act, Andrews and Christenson (1974, p. 374) turn to the weak provisions of the bill. The Bureau of Mines' enforcement powers were limited to safety measures aimed at preventing major disasters, which accounted for less than 10 percent of all fatalities. Furthermore, the legislation did not cover small mines (employing fewer than 15 miners), which made up about 80 percent of all mines. As noted in Equation (4), it is precisely in these small mines that fatalities are more likely to occur. Thus, the 1952 legislation formally put beyond its reach almost all the sources of fatal mining accidents.

Besides this extreme narrowness of scope, which President Truman himself protested, the coal operators and the United Mine Workers of America (UMWA) exhibited a special lack of commitment to its enforcement (McAteer, 1973, pp. 96–101, 181). The companies, worried about falling production, persuaded the UMWA to accept mechanization of the mines in exchange for better wages and benefits. Given the workings of the agreement, signed by John L. Lewis in 1950, the UMWA began to direct itself to the goal of increasing production rather than safety. As Seltzer reports (1974, p. 10), the 1952 contract was succeeded by an end to the adversary relationship with management, and no official strikes occurred before 1972.

The 1952 law was an exceedingly weak law whose enforcement was pursued by neither the union, management, nor the Bureau of Mines. The public, in response to their outrage over the 1951 West Frankfort mine explosion which killed 119 persons, had received only a "symbolic" piece of legislation. However, after its passage, public concern followed the typical pattern of turning to other issues, assuming the mine safety problem was solved. According to Bethell (1972, p. 80), it was not until the Farmington disaster of 1968 that interest in coal mine safety was rekindled. To borrow Bethell's dramatic lines, "It was the beginning of a grim realization: that there were actually people running around who still mined coal for a living, and that they were being killed at their jobs by profit-hungry operators, a corrupted union interested only in the health of its treasury, and a government that seemingly could not care less" (1972, p. 80).

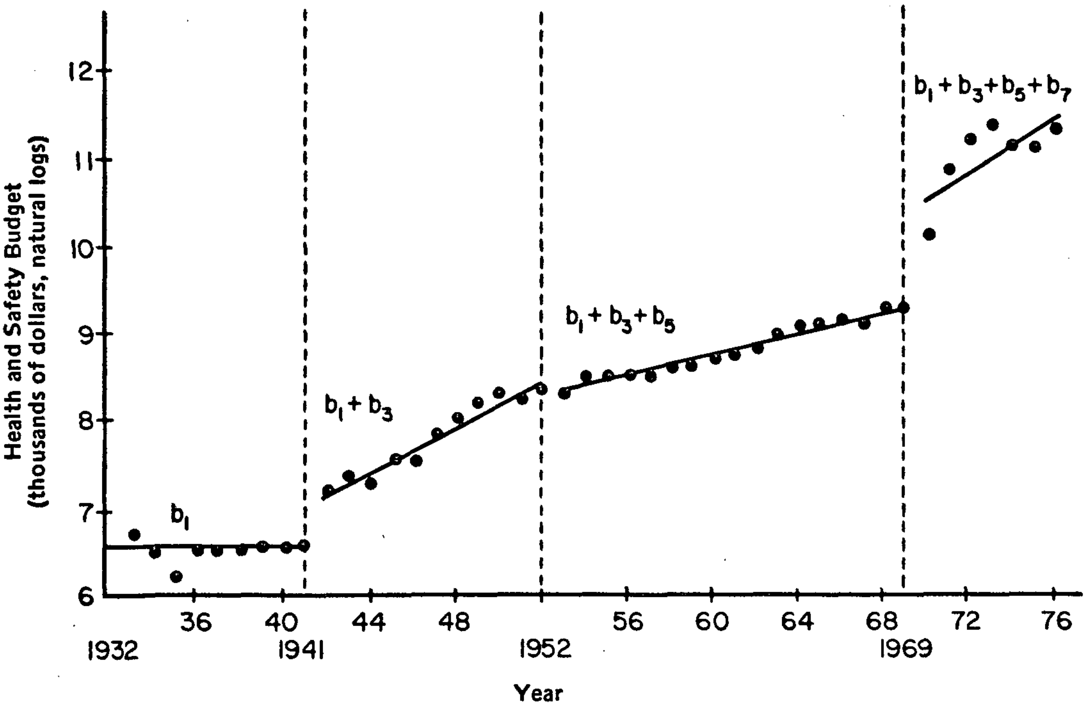
1969: A "Tough" Law. The Federal Coal Mine Health and Safety Act of 1969 is a "tough" law, at least relative to the others. As Harry Caudill, long-time advocate for the Appalachian miner, exclaimed, "For the first time the federal government undertook to rein in and civilize the vast, fragmented, widely dispersed and still ideologically laissez-faire coal industry" (1977, p. 493). In particular, under the 1969 legislation the enforcement powers of the federal inspectors were much expanded and a detailed set of health and safety standards made mandatory for all mines (McAteer, 1973, pp. 213–19). Specification of the standards covers hundreds of pages in the manuals of regulation. These standards have, in the words of mineral economist Richard Newcomb, "changed working practices" in the coal mines and contributed to a more than 30 percent reduction in output per miner (1978, p. 207). By another estimate, eastern deep mining costs have been rising at an annual rate of 10 percent in constant dollars, an increase attributed largely to implementation of the 1969 requirements (Gordon, 1978, p. 155). Thus the stiffer 1969 regulations have advanced their primary goal of providing safer working conditions for miners, even in the face of a costly drop in productivity.

The 1969 legislation seems to offer a long-run solution to the coal mine safety problem, i.e., under its continued enforcement, safety should eventually reach an acceptable level. The strength of the 1969 act is further appreciated when compared to the 1941 act. The latter, while effective, obviously has the weaker provisions. Therefore, we would expect the 1941 law to have less effect than the 1969 law. Indeed, the relevant coefficients from Equation (3) indicate the 1941 law was about half as strong as the 1969 law has been ($b_3/b_7 = .5$). Beyond doubt, the harsher provisions of the 1969 act have made the coal mines safer.

1941, 1952, 1969: Variations in Enforcement. Above we have tried to explain the relative effect of the federal coal mine safety laws. For the most part, we relied on description of the laws and presentation of seemingly relevant facts. It is to be hoped that this exploration enhances understanding of the workings of this legislation. Still, it would be helpful to have more systematic data on the difference in regulation under the three laws. A critical need here is information on enforcement activity. A law, no matter how well written or favored by birth, comes to nothing if it is not actually enforced. The enforcement of coal mining safety legislation, the responsibility of the Bureau of Mines (later MESA), includes such activities as inspections, investigations, rescues, and research (McAteer, 1973, pp. 196–97). A global measure of these activities over time is available–agency expenditures on safety. Of course, a potential problem with the expenditures variable is that, while it summarizes enforcement activities, it may nevertheless be unrelated to reduction in fatalities, which is the goal of enforcement. Therefore, the use of safety expenditures as an overall measure of enforcement is justified only to the extent that it is linked to the fatality rate. Fortunately, this link is quite strong.

If the Bureau of Mines (later MESA) health and safety budget, B_t, is a proxy for federal safety enforcement in the coal mines, we would expect a high negative correlation with the fatality rate, F_t, which it has, $r = -.78$. (When the budget figures are deflated, 1967 = 100, the correlation remains virtually unchanged, $r = -.77$.) While this correlation is substantial, the requisite assumption of linearity is unsatisfactory because the budget variable, like many dollar measures across time, follows an exponential path. Therefore, we applied a natural logarithmic transformation to B_t, in order to "straighten it out." This transformation boosts the correlation with the fatality rate to $-.90$.

Figure 2 plots the annual federal coal mine health and safety budget (logged). The pattern of points indicate that safety expenditures have responded strongly to changes in federal legislation. Observe, for example, the sharp upward trend in spending after 1941, or the big leap up from 1969 to 1970. These shifts are more accurately described when the original MITS model in Equation (3) is fitted to the data. Assuming the independent variables of the model tap real changes in federal regulatory policy, we would expect findings on the budget would reinforce the fatality findings. That is, the vaunted 1941 and 1969 legislative acts would, in the long run, spur safety spending, whereas the maligned 1952 legislation would slow it; specifically, $b_3 > 0$, $b_7 > 0$, $b_5 < 0$. Short-run prediction is more difficult. Equation (3) suggests that the fatality rate was not significantly affected the year after the legislation, which is understandable given the time required to implement the regulations fully. When the dependent variable is health and safety spending, however, one supposes that increases would follow passage of the legislation rather quickly. Therefore, we would expect that the 1941 and 1969 bills would precipitate sudden spurts in the budget, while the 1952 act would cause an immediate budget cut; speci-



Source: Computed from Figures in the *Budget of the United States Government* (1932–1976).

Figure 2. Multiple Interrupted Time Series of Federal Coal Mine Health and Safety Budget (1932–1976)

fically, $b_2 > 0$, $b_6 > 0$, $b_4 < 0$. These predictions are generally sustained in the following OLS equation:

$$\ln B_t = \underset{(58.5)}{6.497} - \underset{(-.4)}{.007X_{1t}} + \underset{(4.1)}{.582X_{2t}}$$

$$+ \underset{(5.7)}{.135X_{3t}} - \underset{(-1.0)}{.125X_{4t}} - \underset{(-3.9)}{.067X_{5t}}$$

$$+ \underset{(7.5)}{1.174X_{6t}} + \underset{(2.4)}{.077X_{7t}} + e_t; \quad (5)$$

$$R^2 = .99 \qquad N = 45 \qquad D\text{–}W = 1.5.$$

The estimates of Equation (5) repay consideration. In the 1930s, the bureau's annual health and safety budget was merely symbolic, averaging just over $500,000, and changing little from year to year (see b_1 and the regression line in Figure 2). However, in the wake of the 1941 act, health and safety appropriations increased significantly, for both the short and long run (see b_2 and b_3). In fact, they began to rise at an average rate of about 13 percent a year ($b_1 + b_3 = .13$).[5] But the 1952 bill, remarkably, elicited no initial budget increment (see b_4), and under it the growth rate was dampened significantly (see b_5). These statistics underline the weakened commitment to mine safety during that period. It took the passage of the 1969 legislation to refurbish the financial commitment to coal mine safety. From 1969 to 1970, the bureau's health and safety funding actually increased over 100 percent. And for all the years subject to the legislation, the rate of appropriation growth has been 14 percent, more than double that of the post-1952 era. (This estimate is actually conservative.)[6]

The collection of MITS variables recording changes in coal mine safety legislation faithfully maps variations in federal enforcement, as measured by the budgetary outlay for coal mine health and safety. Indeed, they account splendidly for such expenditures, as the $R^2 = .99$ indicates. These shifts in the safety enforcement effort mirror the shifts in the fatality rate. Under the stronger legislation (that of 1969 and, to a lesser extent, of 1941), safety expenditures were pushed up and the fatality rate pushed down. In contrast, when the weaker law (the 1952 act) dominated, or there was no law at all (the 1932–1941 period), the safety budget and, consequently, the fatality rate held fairly steady.

Conclusions

Our empirical research began with the question, "Has federal safety legislation reduced the hazards of mining coal?" To provide an answer, we employed multiple interrupted time-series analysis, a neglected procedure in substantive policy investigations. The complex of findings from this analysis would have been predicted by no single political perspective—radical, reactionary, or reformer. While the failure of the 1952 legislation may have surprised reformers, it is congruent with a radical interpretation of the effects of this regulation. However, a radical view does not account for the success of the 1941 and 1969 acts. These efforts demonstrate that government can bend the will of the coal operators, regardless of their quest for profit. Further, these results undercut the reactionary critique, for they show that accidents can be curbed, even in a highly dangerous activity carried out by fallible human beings. Apparently, then, it is possible for Congress to enact laws that successfully regulate American business for some public purpose, such as safer coal mines. Indeed, a 1976 coal miner was over four times less likely to be killed while working than a 1932 coal miner, a fact attributable in no small way to federal safety legislation.[7]

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[5] This percentage change interpretation of the coefficients is possible because of the logarithmic transformation of the dependent variable (if either b_1 or b_3 exceeded .25, the interpretation would become less appropriate). A proof is found in Tufte (1974, pp. 124–26).

[6] The estimate of Equation (5) is conservative, because it undervalues the health and safety appropriations occurring after the move from the Bureau of Mines to MESA in 1974. The health and safety appropriations for MESA are not strictly comparable to the preceding appropriations within the general Bureau of Mines budget intended for health and safety. If we make a reasonable adjustment for this move, our revised estimate of the average annual growth rate for the post-1969 period is 20 percent, rather than 14 percent. Finally, it should be noted that a strong, significant effect of the 1969 legislation (along with the significant effects of the 1941 and 1952 legislation) persists even when these expenditures are deflated (1967 = 100). Thus the budgetary changes detected here are not attributable simply to the force of inflation.

[7] Nevertheless, we should not become complacent about coal mine safety. An idea of coal mining safety advances that could still be made comes from comparing the United States to Great Britain. Unfortunately, the preferred measure, "fatalities per million man-hours," F_t, is not available for the British case.

Why did the 1941 and 1969 safety laws work? Our look at the context, substance, and workings of the legislation suggested some simple, proximate conditions: the *birth order, enforcement*, and *provisions* of those laws. To this brief list, we might add *target population* and *goals*. Let us summarize these conditions as they apply to the legislation at hand. The 1941 act, because it was the first set of rules in a hitherto unruly situation, had an impact which belied its actual content. Further, enforcement of the 1941 law was relatively vigorous, as indicated by the strong increase in the Bureau of Mines' safety budget. Enforcement was still more intense for the 1969 legislation. Moreover, the provisions of the 1969 act, which are comprehensive, detailed, and mandatory, further enhanced its strength. However, beyond these factors, the smallness of the target population has undoubtedly contributed to the success of both pieces of legislation. That is, they aimed to regulate only coal mining, rather than some larger industry or set of industries. Finally, the nature of their goals must account for part of their beneficial effect. Coal mine safety poses a single, narrow, measurable objective. It is not multiple, broad, or difficult to measure, as the goals of legislation sometimes are.

What are the implications of the coal mine safety experience for federal regulation of safety in other workplaces? Specifically, what is suggested with regard to the impact of the OSHA? Certain conditions conducive to effective safety regulation in the mines would also seem operative for the OSHA. Most obviously, it is the first federal legislation aimed at the general regulation of occupational safety. Nevertheless, not too much should be expected from this birth order advantage, since the federal presence remains remote for most businesses. For example, in 1976, the chances were less than 1 in 50 that a workplace would ever be visited by an inspector (Nichols and Zeckhauser, 1977, p. 53). Still another factor favoring the OSHA is the relative strength of its provisions. Thousands of mandatory standards have been developed, spelling out in detail the safety requirements for different workplaces.

In contrast, other conditions appear to operate against the OSHA's success. A critical one is the lack of strong enforcement. Budget figures provide dramatic testimony on the limitation of OSHA enforcement. The 1977 budget was $130 million, little more than the amount provided for enforcement of coal mine safety, despite the fact that with this sum the agency must police a target population consisting of the nation's five million workplaces. Finally, there is a practical problem concerning the detection of the OSHA's success. The goal of occupational health and safety perhaps seems rather specific when compared to the goals of other regulatory agencies. Yet actually measuring its attainment appears a monumental task, when one realizes the size of the target population. In addition, the measurement problem has been compounded by the OSHA legislation, which changed the method of collecting data on injury rates, thus making pre- and post-OSHA comparisons problematic.

The future of federal policy toward occupational safety is difficult to foresee. However, any politically acceptable approach, whether interventionist or noninterventionist, will probably not have a dramatic effect on the accident rate in the American workplace, at least in the short run. Nichols and Zeckhauser (1977, p. 66), in their discussion of the OSHA, remark, "As distasteful as it may be, given the competing claims for resources, we may have to accept a significant level of occupational accidents as a cost of doing business." Nonetheless our research indicates that at least under some conditions government *can* regulate safety. Further, in regard to the OSHA our research on coal mining safety suggests that direct OSHA intervention would be more successful if it confined itself to industries with "high risk" jobs. That is to say, the OSHA should make a special effort to identify occupations which, like coal mining, are unusually hazardous. Then, it should concentrate its resources, which will undoubtedly remain limited, on making those dangerous occupations as safe as work in other industries.

However, a measure of "fatalities per thousand employed," E_t, does exist. (For the United States data, E_t correlates .86 with F_t, indicating considerable empirical overlap between the two.) In the United States, for the seven-year period after the 1969 legislation, average $E_t = 1.02$, *a safety figure achieved in Great Britain over 50 years ago* (see Hair, 1968, p. 545). The most recent British figure (1974), which applies strictly to underground mining, is .19 (Rothschild, 1979, p. 20). When compared to the United States overall estimate for 1974 of .75, this implies the risk of death is about four times greater in an American coal mine.

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The Emergence of Political Parties in Congress, 1789-1803

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Although the political leaders who wrote the Constitution did not hold the idea of party in high regard, these same individuals (according to many historians) became the founders of a new party system within the first decade of the new government. This article considers the question (on which no consensus exists) of whether parties did develop. The analysis focuses upon one aspect of party development, namely, the agreement among members of Congress in their roll-call voting records. Spatial analysis (multidimensional scaling) permits a visual picture of the increased clustering of congressmen into two party blocs from 1789 to 1803, especially after the Jay Treaty debate in 1796. This very clear trend supports the idea that politics was moving away from a sectional basis to one founded more clearly on partisan grounds.

Scholars have differed considerably in assessing the development of American political parties during the period immediately following ratification of the Constitution. The date marking the emergence of parties has been placed anywhere between the beginning of the new government and the time 50 years later when Whigs and Jacksonian Democrats were competing for power. Despite disagreements over the precise date when parties first appeared, most observers have agreed that important divisions did materialize in the first decade of the new nation. Varying explanations have been offered for these divisions, ranging across economic factors, regionalism, personalities, issues, and even the boardinghouse residences of members of Congress.[1]

This article explores the question of when and why political parties first appeared in the United States. I examine the development of parties for the period between 1789, the first year of the new government under the Constitution, and 1803, a year which directly followed the first transition of government from one party to another. This span of 14 years is of particular interest because it was dominated by individuals who themselves did not believe parties could be a positive force in a governmental system. It was also a critical era of national development when the United States was trying to establish itself as a newly independent nation under a democratic government, a form generally untested at that time.

To reach conclusions concerning party development which may resolve some of the confusion in the literature, I have undertaken two tasks. First, I examine carefully the concept of "party" and distinguish it from the idea of "faction." Second, I apply a new methodological approach to consider empirical evidence on the question of party development. I use spatial analysis (multidimensional scaling) of roll-call voting because it avoids some of the limitations in other methods of roll-call analysis. I then examine spatial maps of congressional voting to answer questions concerning the state of party development in the early years of the nation.

An earlier version was presented at the 1976 meetings of the Southern Political Science Association, Atlanta. I have benefited greatly from the comments of many friends from Chapel Hill, especially Beth Fuchs, George Rabinowitz, Bill Keech, Duncan MacRae, Forrest Young, Don Searing, Whit Ayres, Ed Crowe, Nelson Dometrius, as well as several referees. Acknowledgement is also due to the Historical Archive of the Inter-University Consortium for Political and Social Research for the compilation of roll-call votes and to the Institute for Research in Social Science at the University of North Carolina at Chapel Hill for computing support. Nevertheless, tradition requires me to take responsibility for all remaining errors.

[1] According to one traditional interpretation of American party development, political parties existed from the beginning of the national government (Beard, 1915; Ryan, 1971). Other scholars have cited the Jay Treaty as the issue which marked appearance of parties, thus placing the date at about 1795 (Bell, 1973; Chambers, 1963; Charles, 1961; Cunningham, 1957). Still others have pointed to events and issues during the Adams administration as crucial in the emergence of parties (Dauer, 1953; Libby, 1912). Finally, a group of party historians has argued that true parties did not exist in this early period and has placed the date of party emergence after 1830 (Formisano, 1974; Hofstadter, 1969; Nichols, 1967). In many cases, these different conclusions can be attributed to varying definitions of party and an assortment of methodological approaches.

Toward a Definition of Party

As of 1789, a clear distinction between party and faction had not been established. According to eighteenth-century usage, "faction" (for which the word "party" was often substituted) usually referred to a group involved in the political arena but working toward private ends. Party, on the other hand, was "a new name for a new thing" (Sartori, 1976, p. 64). As the idea of party developed in the seventeenth and eighteenth centuries, the word gradually acquired a more distinct and positive meaning than "faction." That this distinction was not commonly recognized in 1789 is illustrated by the writings of James Madison in *The Federalist,* where he cautioned against both the "violence of faction," and the "rage of party." Although he used the terms interchangeably, he clearly had in mind the old idea of faction and not the new idea of party (Hofstadter, 1969; Sartori, 1976).[2]

Today the distinction between "party" and "faction" is much clearer. "Faction" tends to be used in two different contexts. In one, it retains the negative connotations of the old concept, referring to a self-seeking and contentious group. In a nonpartisan setting or within a single party, it may denote a group which forms around a single issue or personality, but with limited durability and minimal organization. A political party, on the other hand, is a more permanent group which generally has a positive role in the political system. Parties "are instrumental to collective benefits. . . . [They] are functional agencies–they serve purposes and fulfill roles–while factions are not" (Sartori, 1976, p. 25). There are in fact two important and strongly related roles which can be fulfilled by parties. One is an involvement in elections. Parties may help to recruit and screen candidates, supply symbols and names to help identify candidates to voters, and provide victorious candidates a mechanism for working toward policy goals once in office. The other role is serving as what Sartori calls "channels of expression" (p. 27). The party is one institution which helps people communicate their demands to the government. Furthermore, because a party can simultaneously channel the demands of a number of people, it adds a degree of pressure which aids popular control of public policy.

While these roles are normally associated with the concept of party, the term itself is generally defined from a more structural perspective. Although numerous definitions of "party," emphasizing a variety of themes, can be found in the literature, several components are common to most of them. The following elements of the concept should be considered in any investigation of party development.[3] All parties have:

1. a common symbol or label,
2. a group of leaders in office,
3. a group of supporters in elections,
4. an organization, however minimal.

Several theories have been put forth to explain how parties come into being (Chambers, 1966; Duverger, 1959; Huntington, 1968; LaPalombara and Weiner, 1966). Integrating these considerations leads to the view that party development is a process taking place in four stages.[4] In the first stage, *factionalism,* groups form in the legislature over a variety of issues and personalities.[5] These factions are rarely organized and last for only a short time. In the second stage, *polarization,* these factions are stabilized into more permanent legislative groups which oppose each other over a broad range of issues. This coalescence into polar groups is frequently set off by a single issue of overriding importance or by the cumulation of several cleavages. In the third stage, *expansion,* the public is drawn into the process of party development, usually after the extension of suffrage. Electoral committees may arise at the local level to influence decision makers or may be created by office holders to strengthen their own positions. In either case, at this stage party affiliations become significant factors in the electoral process. In the fourth and final stage, *institutionalization,* a permanent linkage is created between the parliamentary group and its electoral committees. At this point some type of national organization should begin to direct and coordinate the party activities.

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[2] References are taken from *The Federalist,* No. 10 (Madison et al., 1961, p. 77) and No. 50 (p. 320). Elsewhere Madison clearly shows that he equates the two words, referring to "the most numerous party, or in other words, the most powerful faction. . ." (No. 10, p. 80).

[3] This discussion on definitions of party was strongly influenced by comments of Formisano (1974, p. 475), who in turn drew heavily from Chambers (1967) and Sorauf (1967), whose ideas were also important in the present effort. An alternative definition is provided by Sartori (1976, p. 63).

[4] Huntington's terminology (1968, pp. 412–20) is used here, but the meaning of the stages has been modified to incorporate other views, particularly Duverger's (1959) idea of the parliamentary origin of parties.

[5] This use of "factionalism" is not intended to carry the negative connotations often attached to the term.

This article examines the transition of American political parties from factionalism to polarization and therefore focuses on parties emerging within the Congress. The expansion of partisanship into the electorate and the institutionalization of parties are considered only where they amplify the party development taking place in Congress.

Methodology

The principal data for this investigation are the records of how members of Congress voted on roll calls taken between 1789 and 1803. While there are inherent limitations in using roll calls, the official nature of these votes makes them a valuable source of information.[6] The chief analytic method is multidimensional scaling of measures of agreement between legislators. Since this method has been fully discussed elsewhere,[7] I will give only a brief summary of the approach. Multidimensional scaling employs a matrix of agreement scores, defined simply as the proportion of times two legislators agree in their votes, out of the total number of bills on which both vote. These agreement scores are transformed into distances in a geometric space of some given dimensionality. The legislators are then represented as points in this multidimensional space in such a way that those who *agree* most often in voting are *closest* to each other in the resulting configuration of points, while those who *disagree* most are *farthest* away in the space.

Interpretation of a multidimensional scaling configuration is perhaps the most important and most difficult part of the analysis. First, the quality of a solution, or the fit between the data (agreement scores) and the configuration, is indicated by a statistic called "stress," which measures the degree to which the configuration fails to reproduce accurately the relationships present in the data. Stress can range theoretically from 0.0, for a perfect fit, to 1.0, for a total lack of fit. Second, the appropriate dimensionality of a solution must be determined with respect to its stress. A solution can be derived in any number of dimensions, and the stress will always be lower when a higher dimensionality is allowed. Thus the analyst must determine the most appropriate dimensionality, according to the conflicting standards of good fit (low stress) and parsimony (a small number of dimensions). Finally, the interpretation of a configuration involves a search for meaningful dimensions, clusters, or other structural patterns. It must be emphasized that a two-dimensional solution should *not* automatically be discussed in terms of two linear dimensions, such as a factor analysis would be. The analyst must be attentive in searching for that structural interpretation which best represents the information contained in the scaling solution.

An alternative method, cluster analysis, is used to aid the interpretation and explication of the configurations derived by multidimensional scaling. In clustering, legislators are placed into blocs or clusters, based on their level of agreement with each other. Those grouped in a particular cluster are more likely to agree with others in that cluster than with those outside it. Once clusters have been obtained, it is possible to compute the average level of agreement within them. Because cluster analysis is based on the same agreement scores, the results should help to support conclusions reached from multidimensional scaling.

While cluster analysis can be used to discover blocs that exist on the basis of voting patterns, the cohesion of parties and sectional blocs, defined externally to the voting records, is of equal interest. For each group, I calculate several standard indices of partisanship, including the average level of agreement, Rice's index of cohesion, Rice's index of difference, and Lowell's party vote.[8]

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[6] Some limitations of roll-call analysis are immediately obvious. Roll-call votes may not always be accurate records of either the scope of activity in Congress or the true positions of members. Also, for a variety of reasons, roll calls are not taken on every issue before the legislature. Furthermore, when votes are recorded, members may sometimes misrepresent their positions, for reasons such as friendship or future reelection. Roll calls nevertheless do provide an official record of positions taken on a variety of issues and thus may be more important than a legislator's own preferences in cases where they differ.

[7] A more detailed presentation and justification of the methodology used here is found in Hoadley (1979). A good general discussion of multidimensional scaling is provided by Rabinowitz (1975). The use of cluster analysis for studying legislative blocs has been evaluated by MacRae (1970).

[8] Rice's index of cohesion is the absolute difference between the percentage "yes" and percentage "no" for a particular group. His index of difference is the absolute difference between the percentage "yes" for any two groups. A party vote, according to Lowell, is one where more than 90 percent of one party opposed more than 90 percent of the other. MacRae (1970, pp. 177–84) provides a fuller discussion of these indices.

Another means of studying party development is to consider the party identification of congressmen. This process is, however, far more difficult in studying the eighteenth century than the twentieth, since party labels were not commonly used in those early years of our nation. There were no formal party caucuses in Congress, nor were party labels used in most congressional elections. Nevertheless, information on the party affiliations of members of Congress has been compiled in the historical archives of the Inter-University Consortium for Political and Social Research (ICPSR), the *Biographical Directory of the American Congress, 1774–1961* (U.S. Congress, 1961), and several monographs on the early party system (Bell, 1973; Dauer, 1953). This information remains incomplete and occasionally inconsistent, particularly for the earliest years.[9] Nevertheless, party labels can aid in the assessment of partisan development. The extent to which congressional voting blocs correspond to those labels is one indication of the emergence of a party system.

Spatial Analysis of Roll-Call Voting

Once the scaling and clustering solutions are obtained, it is necessary to assess whether the resulting configurations show the existence of parties. As I have indicated, this article concentrates on the stages of factionalism and polarization. In the factional stage, congressional voting is expected to be fairly unstructured. Stress should be relatively high, as votes are cast for idiosyncratic reasons. Furthermore, the legislators should not be divided into two or three very distinct blocs. Since different issues presumably produce different divisions, the individual points should be distributed fairly uniformly across the space.

As party development evolves from factionalism to polarization, the configurations should change correspondingly. In the second stage, groups of legislators are expected to be voting together across a set of issues, rather than shifting alignments from one issue to the next. Thus the configurations should reveal much tighter clusters of legislators corresponding to these party groups. Of course, the lines of division may still shift somewhat from one issue to another, but there should be a clear consistency of groupings, which was not true in the earlier stage of development. It is impossible to denote specific thresholds of agreement or distinctive patterns which would mark the existence of a party, for many factors affect these results. But careful examination of patterns over time should allow reasonable conclusions to be drawn regarding trends in partisanship.

With the establishment of these criteria for party development, I can examine the multidimensional scaling analysis of congressional voting. Configurations have been generated in one, two, and three dimensions for both the Senate and House of Representatives of the First through the Seventh Congresses (1789–1803). In each case, all roll calls were included and weighted equally,[10] and all members were included except those who were absent on a large proportion of roll calls. The stress for each configuration, with the number of legislators and number of roll calls involved, is presented in Table 1. Configurations for the House are presented in Figures 1 through 5, and Senate configurations for illustrative years[11] are presented in Figures 6 through 9.

The two-dimensional configuration has been selected as the best representation of voting patterns in each congress, with the sole exception of the First Senate. For several congresses, three-dimensional solutions yield a distinct improvement in stress, yet in every case but one the added accuracy is counteracted by increased difficulty in visualizing the resulting configuration. In the case of the First Senate, however, a two-dimensional configuration would not accurately represent the patterns of voting agreement, and a three-dimensional configuration is presented instead. One-dimensional configurations would be adequate in some cases on the basis of stress, but there is no advantage in restricting the figures to a single dimension.

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[9] It is clear, from comparisons with primary research reported in several monographs on state party development, that there are frequent errors in the ICPSR file and in the *Biographical Directory*. I have estimated an error rate of greater than 10 percent for some years, and that complete and accurate information is generally available for only about 60 percent of the congressmen. A compilation of party labels from primary sources is needed, indicating the party with which a member of Congress identified at the time of each election. In the absence of such a compilation, the party codes used in this article represent the best information available in published sources.

[10] The inclusion of all roll calls with equal weight may introduce a bias into the analysis, although its degree and nature are unknown. Because any scheme of exclusion or weighting would be difficult to justify, all votes have been included. Given the nonmetric assumptions of MDS and the strength of the results, it seems unlikely that any resulting bias has seriously affected the conclusions.

[11] Configurations for the Third, Fifth, and Sixth Senates are not included here, to conserve space. Patterns for these years differ little from those of surrounding years, and the configurations fit clearly within the overall pattern of party development.

It is important to reemphasize that these configurations are *not* to be interpreted in terms of two orthogonal linear dimensions. Thus I have not labeled the vertical or horizontal dimensions of the figures. Although such labeling can be appropriate in certain instances, it does not provide the best description of patterns in the period under consideration. Rather, the configurations should be seen as clusterings of legislators in a two-dimensional (or three-dimensional) space. A closely grouped cluster of congressmen indicates a cohesive voting bloc, and the relationships among voting blocs and unattached legislators are displayed in the configurations.

The series of configurations across the seven congresses, both in the House and the Senate, provides convincing evidence for the development of political parties during this period. The pattern which emerges is a clear progression from factionalism to polarization. In the following sections, the configurations are examined in greater detail.

Factionalism, 1789–1791

After the new Constitution was proposed at the Philadelphia Convention in 1787, it had to be ratified by at least nine states to become effective. The ensuing process involved considerable disagreement and very close votes in several states. This conflict between supporters and opponents of the Constitution became the major issue affecting the elections for the First Congress in 1788 and 1789. While personalities and local issues were often decisive, nearly every state experienced contests between Federalists and Antifederalists. The Federalists, who generally had more incentive to become involved in the new government, were victorious in most contests. In fact, only 10 (of 65) seats in the House and only 2 (of 26) seats in the Senate were won by Antifederalists (Paullin, 1904).

Despite the importance of divisions over the Constitution, this issue did not dominate voting in the First Congress. While Antifederalist members of the House are separated roughly at the top of the voting configuration (Figure 1), this division clearly was not the major one. This is illustrated by noting that the average agreement between Federalists and Antifederalists (49.3 percent) was not much below the average agreement within either group (Federalists, 55.1 percent; Antifederalists, 57.9 percent).

To the extent that there was any consistency in voting patterns, it was along sectional lines. In the First Senate, there is minimal evidence of consistent voting (Figure 6). Those alignments that do exist, which require three dimensions to represent spatially, are geographical and cannot

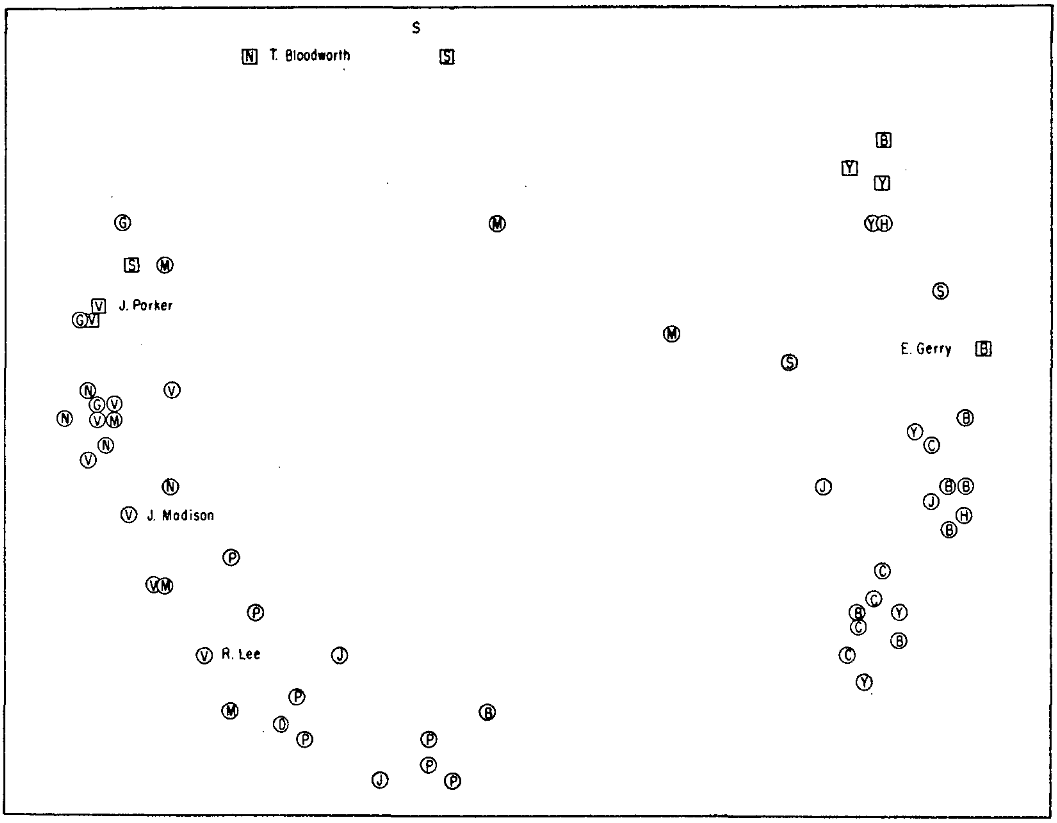
Table 1. Quality of Fit between Agreement Scores and Spatial Configurations (in 1, 2, and 3 Dimensions)

Congress	Level of Stress			Number of Members	Number of Roll Calls
	1 Dim.	2 Dim.	3 Dim.		
House of Representatives					
First (1789–1791)	.468	.337	.263	62	109
Second (1791–1793)	.438	.304	.249	65	102
Third (1793–1795)	.278	.237	.201	100	69
Fourth (1795–1797)	.282	.233	.213	100	83
Fifth (1797–1799)	.084	.070	.066	100	155
Sixth (1799–1801)	.059	.055	.053	100	96
Seventh (1801–1803)	.041	.040	.040	100	141
Senate					
First (1789–1791)	.711	.489	.275	26	100
Second (1791–1793)	.420	.275	.185	27	52
Third (1793–1795)	.278	.191	.159	29	79
Fourth (1795–1797)	.179	.161	.142	30	86
Fifth (1797–1799)	.202	.162	.150	31	202
Sixth (1799–1801)	.207	.173	.151	36	120
Seventh (1801–1803)	.107	.097	.091	33	88

Source: Derived from multidimensional scaling of recorded roll-call votes in the United States Congress (collected by the Inter-University Consortium for Political and Social Research).

be considered evidence of party voting. With certain exceptions, senators from New England, the Middle States, and the South are each isolated in a distinct region of the space. The exceptions, however, foreshadow the coming party alignment. William Maclay, from Pennsylvania's more developed party system, is aligned rather closely with Virginia's senators, an alignment consistent with the future Jeffersonian Republican party.

While it is difficult to find even factional patterns for voting in the First Senate, voting in the House of Representatives is far more structured (Figure 1). Although the stress in two dimensions is not perfect, it is far better than the stress for a two-dimensional representation of the Senate. Evident from either multidimensional scaling or cluster analysis are three principal voting blocs, which are best described as sectional groupings. One bloc



Source: Spatial configuration, derived from multidimensional scaling of recorded roll-call votes in the United States Congress (collected by the Inter-University Consortium for Political and Social Research).

Key: Each symbol indicates the state represented by a particular congressman. The circle or square drawn around the letter indicates party affiliation.

B = Massachusetts
C = Connecticut
E = Vermont
G = Georgia
H = New Hampshire

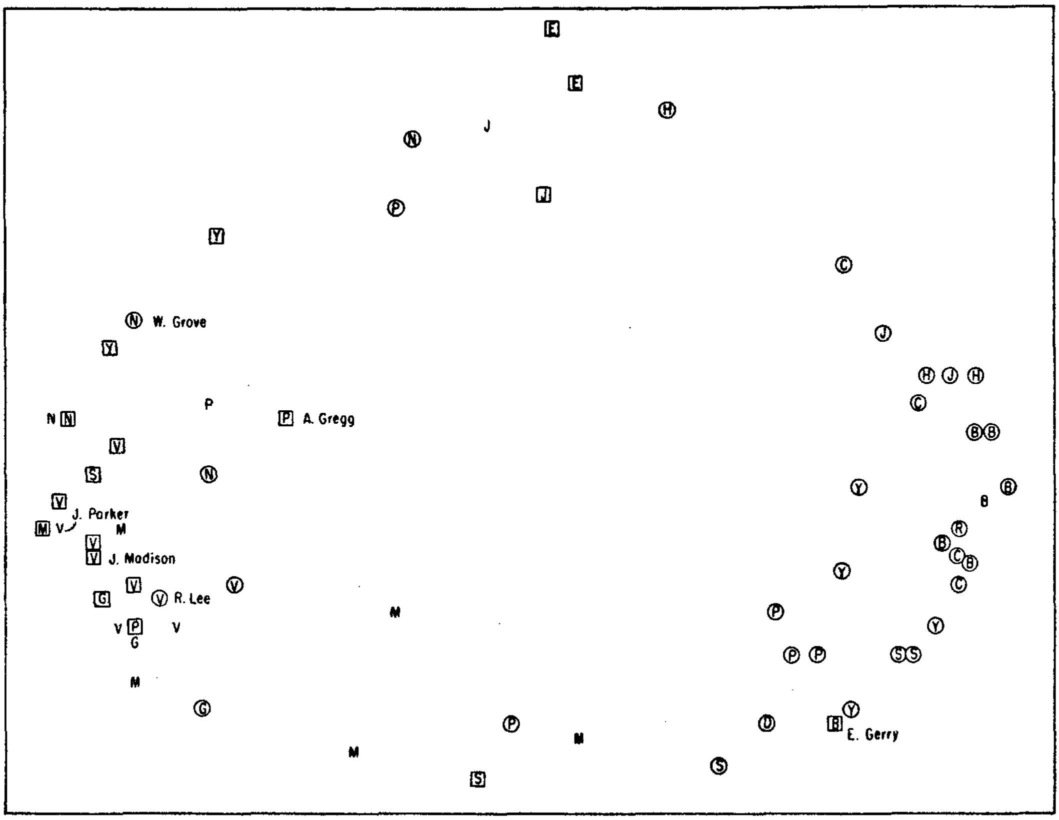
J = New Jersey
K = Kentucky
M = Maryland
N = North Carolina
P = Pennsylvania

R = Rhode Island
S = South Carolina
T = Tennessee
V = Virginia
Y = New York

Ⓧ = Federalist
🅇 = Antifederalist
X = None/Unknown

Figure 1. Voting Patterns in the First Congress, United States House of Representatives, 1789–1791

includes mostly members from New England and New York. The second consists almost exclusively of southern congressmen, and the third is dominated by members from Pennsylvania and several neighboring states. The principal exceptions to this alignment come from New Jersey, Maryland, and South Carolina. The four New Jersey congressmen are divided between the New England and Pennsylvania blocs, according to their proximity to the cities of New York and Philadelphia. Maryland congressmen are placed with or near each of the three voting blocs, perhaps in accordance with the state's location between North and South. Finally, the South Carolina delegation contained two members (both representing coastal towns, Charleston and Georgetown) who voted in agreement with the New England bloc. Throughout the early years, South Carolina had the strongest Federalist party of any southern



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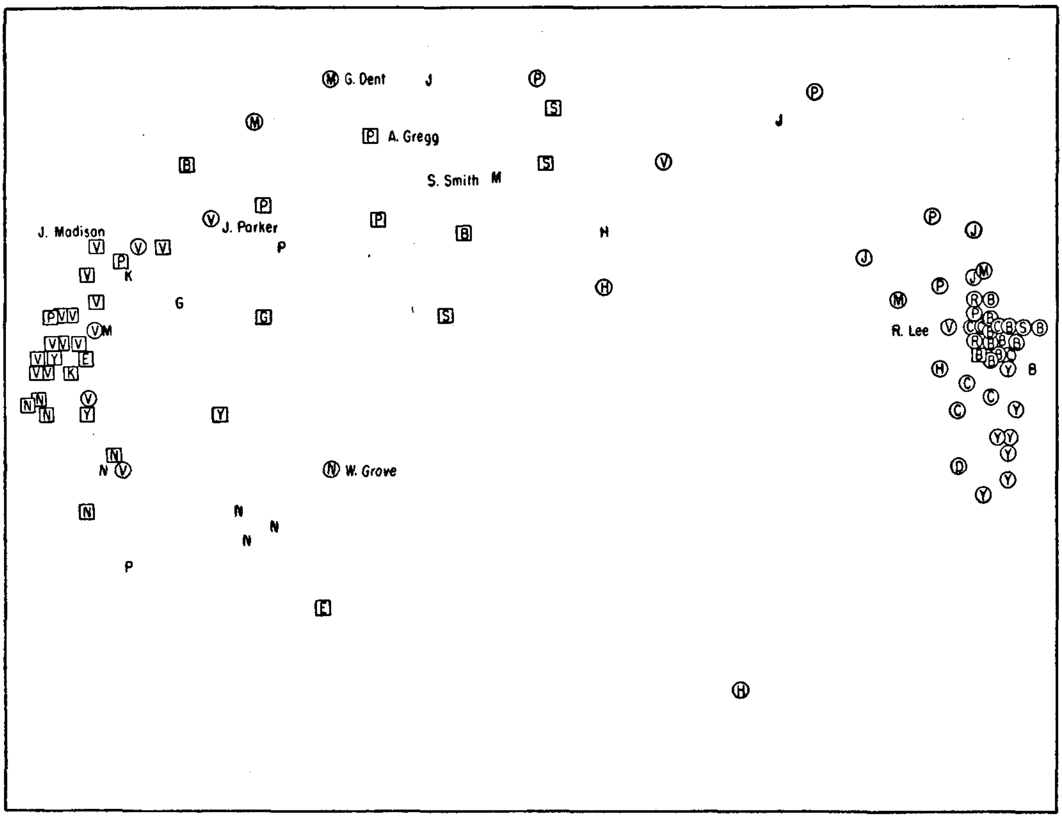
T = Tennessee
V = Virginia
Y = New York

Ⓧ = Federalist
$\boxed{X}$ = Republican
X = None/Unknown

Figure 2. Voting Patterns in the Second Congress, United States House of Representatives, 1791–1793

state, a fact best explained by the existence of trading interests centered in the port of Charleston.

These patterns of congressional voting in the House provide evidence that factionalism was present. At least two major sets of issues in the First Congress contributed to the appearance of this stage. One was the location of the new capital. The choice between New York, Philadelphia, the Susquehanna valley, and the Potomac valley contributed to the sectional divisions, particularly the separation of the various Middle States delegations. Another large set of votes in the First Congress concerned domestic economics, specifically relating to Hamilton's fiscal proposals. These votes have been cited as the first on which new partisan alignments arose, and they also demonstrate the lack of continuity between the old divisions over the Constitution and the newly emerging divisions. While majorities of both the North Carolina and Virginia delegations had been supporters of the



Source: Spatial configuration, derived from multidimensional scaling of recorded roll-call votes in the United States Congress (collected by the Inter-University Consortium for Political and Social Research).

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P = Pennsylvania
R = Rhode Island
S = South Carolina
T = Tennessee
V = Virginia
Y = New York

Ⓧ = Federalist
☒ = Republican
X = None/Unknown

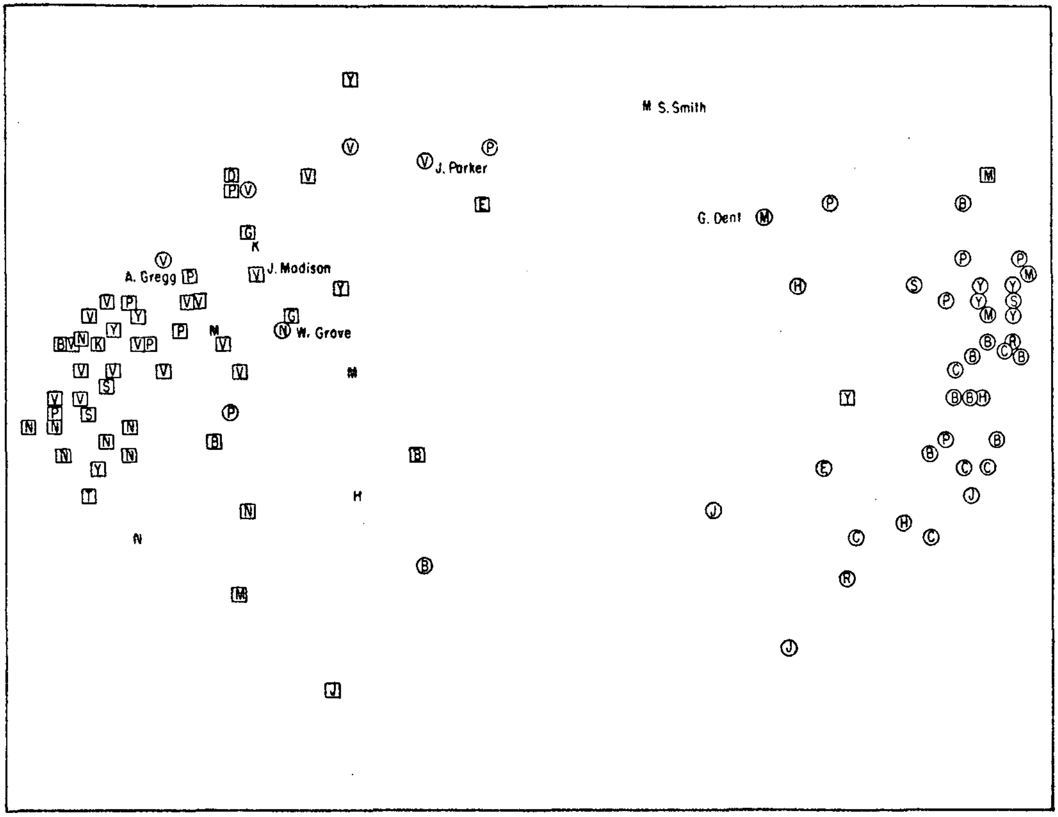
Figure 3. Voting Patterns in the Third Congress,
United States House of Representatives, 1793–1795

Constitution, they were "federalists only in support of the Constitution and not federalists in upholding all of the centralizing measures of the new government" (Gilpatrick, 1931, p. 45).

A Period of Transition, 1791–1793

By the Second Congress, voting patterns were still best described as factional, but sectionalism was beginning to give way to partisanship. There was not yet, however, a clear movement toward polarization of legislators into two cohesive groups. Nor was there much evidence of partisanship in elections for this congress.

The circular pattern evident in the configuration for the Second Senate represents a transitional stage in the development of party voting (Figure 7). The circle actually consists of four distinct blocs of senators plus one isolated individual (Aaron Burr). These blocs are clearly dominated by senators from particular regions: a South bloc, a South-plus-Middle bloc, a



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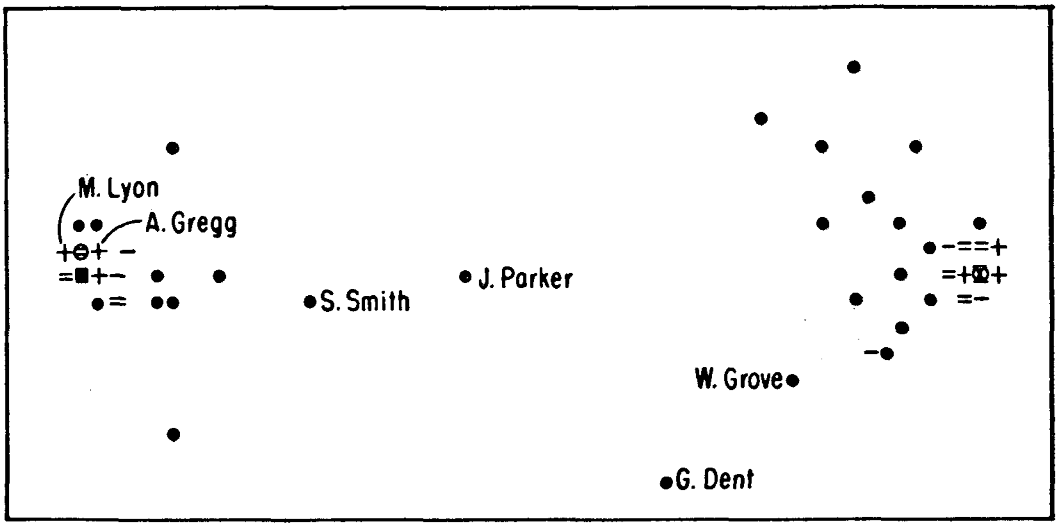
T = Tennessee
V = Virginia
Y = New York

Ⓧ = Federalist
🅇 = Republican
X = None/Unknown

Figure 4. Voting Patterns in the Fourth Congress, United States House of Representatives, 1795–1797

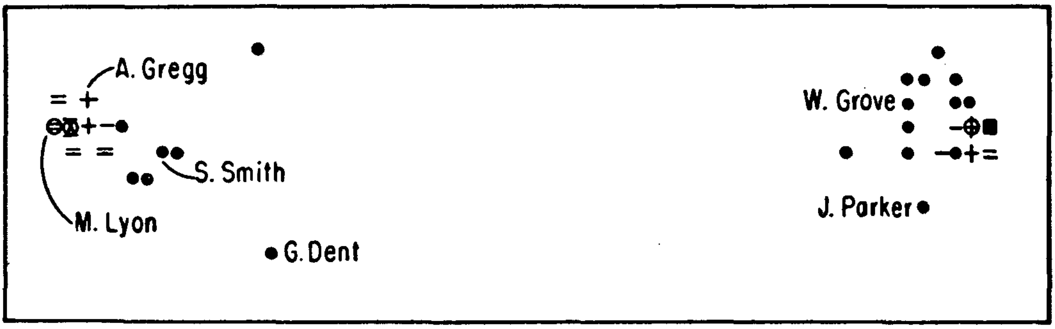
A. Fifth Congress, 1797–1799



Key: Each symbol represents the number of congressmen at a particular point in the space.

Symbol	Number of Members	Symbol	Number of Members
●	1	+	4
–	2	⊜	5
=	3	⊗	7
		■	9

B. Sixth Congress, 1799–1801



Key: Each symbol represents the number of congressmen at a particular point in the space.

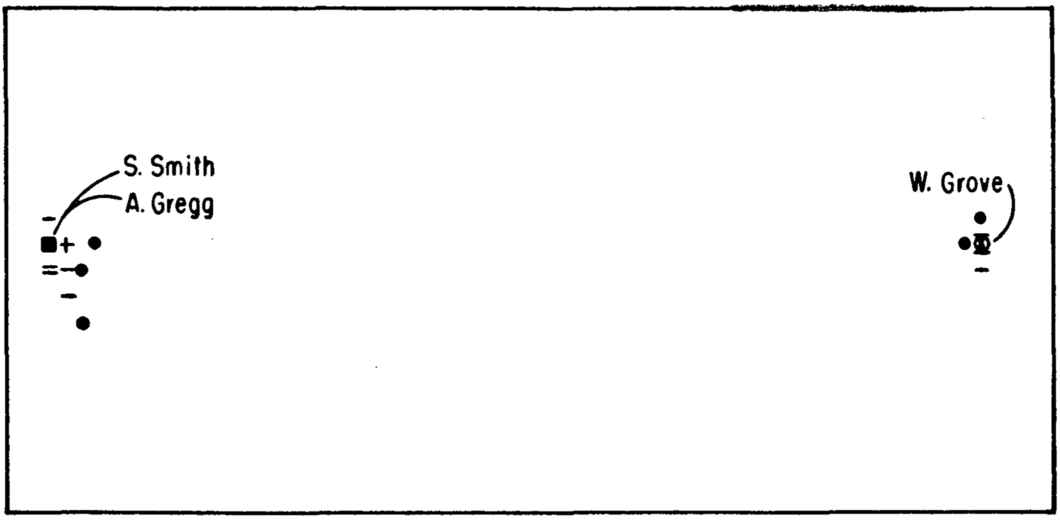
Symbol	Number of Members	Symbol	Number of Members
●	1	⊜	7
–	2	⊕	9
=	3	⊗	16
+	4	■	19

Source: Spatial configuration, derived from multidimensional scaling of recorded roll-call votes in the United States Congress (collected by the Inter-University Consortium for Political and Social Research).

Figure 5. Voting Patterns in the United States House of Representatives, 1797–1803

Figure 5. Voting Patterns in the United States House of Representatives, 1797–1803, ***continued***

C. Seventh Congress, 1801–1803



Key: Each symbol represents the number of congressmen at a particular point in the space.

Symbol	Number of Members	Symbol	Number of Members
●	1	+	6
–	2	⊗	34
=	4	■	43

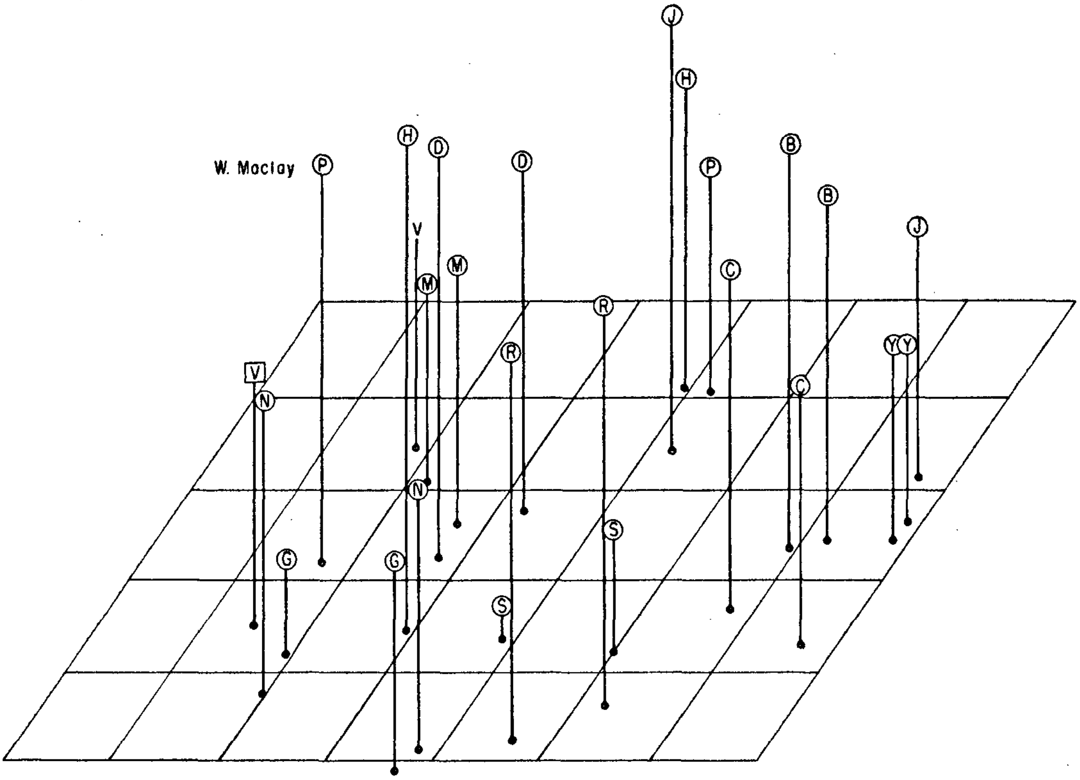
Middle-plus-New England bloc, and a New England bloc. Furthermore, the third of these blocs consists exclusively of senators who were being identified as Federalists or were friendly toward Hamilton's fiscal programs. While no bloc is exclusively Republican, there is a tendency for Republicans to be at the opposite side of the figure from the Federalist core group. Clearly, voting in the Second Congress was still more determined by sectional groupings than by party, but the origins of the two parties are already evident in the configuration for this Senate.

Aaron Burr's isolated position is particularly intriguing given his special role in early American politics. His position in the spatial configuration is even more isolated than it appears in Figure 7. In a three-dimensional configuration, Burr alone defines the third dimension, showing his individualized voting in this congress. Chosen as a senator from New York because of a "reputation for independence which fascinated substantial men of both parties" (Young, 1967, p. 189), Burr's record in the Second Congress was truly one of independence from partisanship. Only in later years did he become a confirmed Republican, both in his congressional voting and in his political ambitions.

The configuration for the Second House (Figure 2) is not very different from that of the First House. The principal features are still the regional blocs, particularly the New England and southern blocs. Once again, the South Carolina delegation provided the main exceptions, with the continuing alliance of those from coastal districts with northern congressmen. A second notable exception was the two-member delegation from the newly admitted state of Vermont, the first state without direct access to the coast.

Delegations which changed the most from the First to the Second Congress were those from the Middle States. No longer was each state delegation a cohesive voting bloc. The New York and Pennsylvania delegations had become internally divided, with three Pennsylvanians in both the southern and northern blocs, and two others between the two blocs. In New York's delegation, two members aligned themselves with the South, and the other four voted with New England. The New Jersey delegation remained divided, but no longer along geographic lines. Finally, the Maryland delegation was still much closer to the southern bloc, but only half of it could be considered part of that bloc.

Sectionalism remained the dominant cleavage in the Second Congress. Yet geographic lines were becoming less important in certain states, especially New York and Pennsylvania. Whether this new diversity was truly a sign of partisanship, however, remains a question. In New York, most candidates could be identified as Federalists or Antifederalists, and victorious candidates later voted consistently with these affiliations. But the elections did not generally present clear partisan choices, and it would be at least two more years before the Antifederalists became known as Republicans (Young, 1967). In Pennsylvania, the 1791 elections were held on a district basis and were characterized



Source: Spatial configuration, derived from multidimensional scaling of recorded roll-call votes in the United States Congress (collected by the Inter-University Consortium for Political and Social Research).

Key: Each symbol indicates the state represented by a particular congressman. The circle or square drawn around the letter indicates party affiliation.

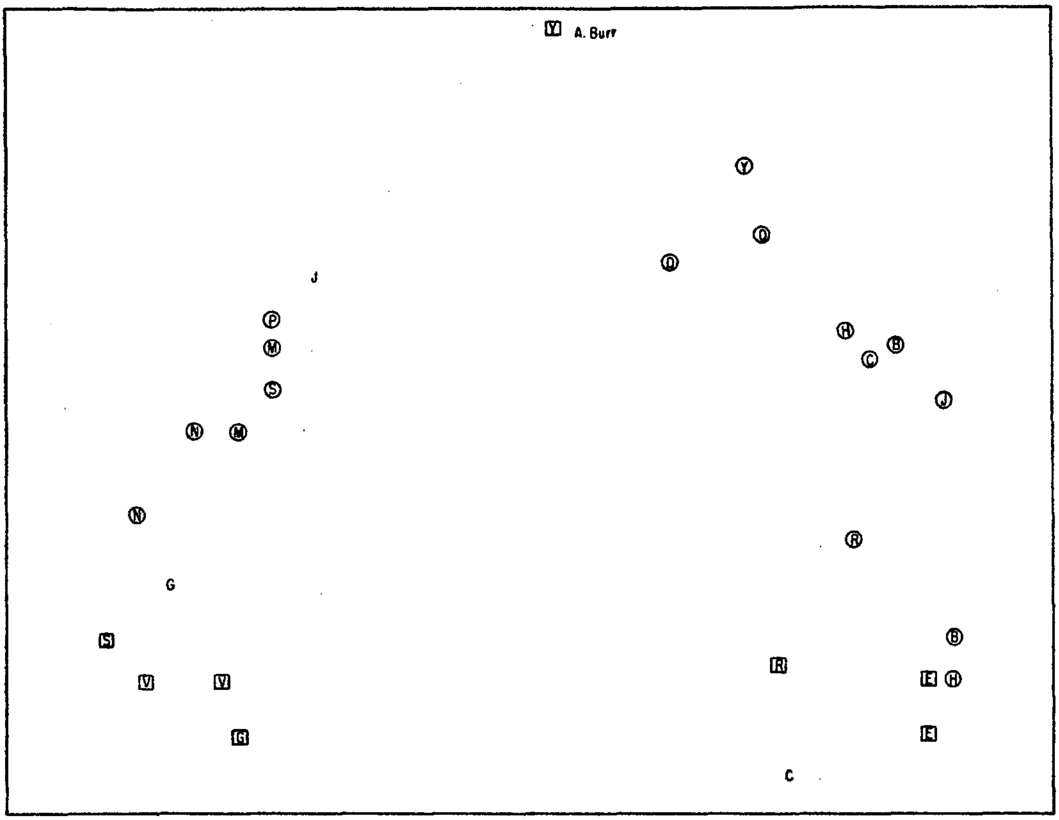
B = Massachusetts
C = Connecticut
E = Vermont
G = Georgia
H = New Hampshire

J = New Jersey
K = Kentucky
M = Maryland
N = North Carolina
P = Pennsylvania

R = Rhode Island
S = South Carolina
T = Tennessee
V = Virginia
Y = New York

Ⓧ = Federalist
☒ = Antifederalist
X = None/Unknown

Figure 6. Voting Patterns in the First Congress, United States Senate, 1789–1791



Source: Spatial configuration, derived from multidimensional scaling of recorded roll-call votes in the United States Congress (collected by the Inter-University Consortium for Political and Social Research).

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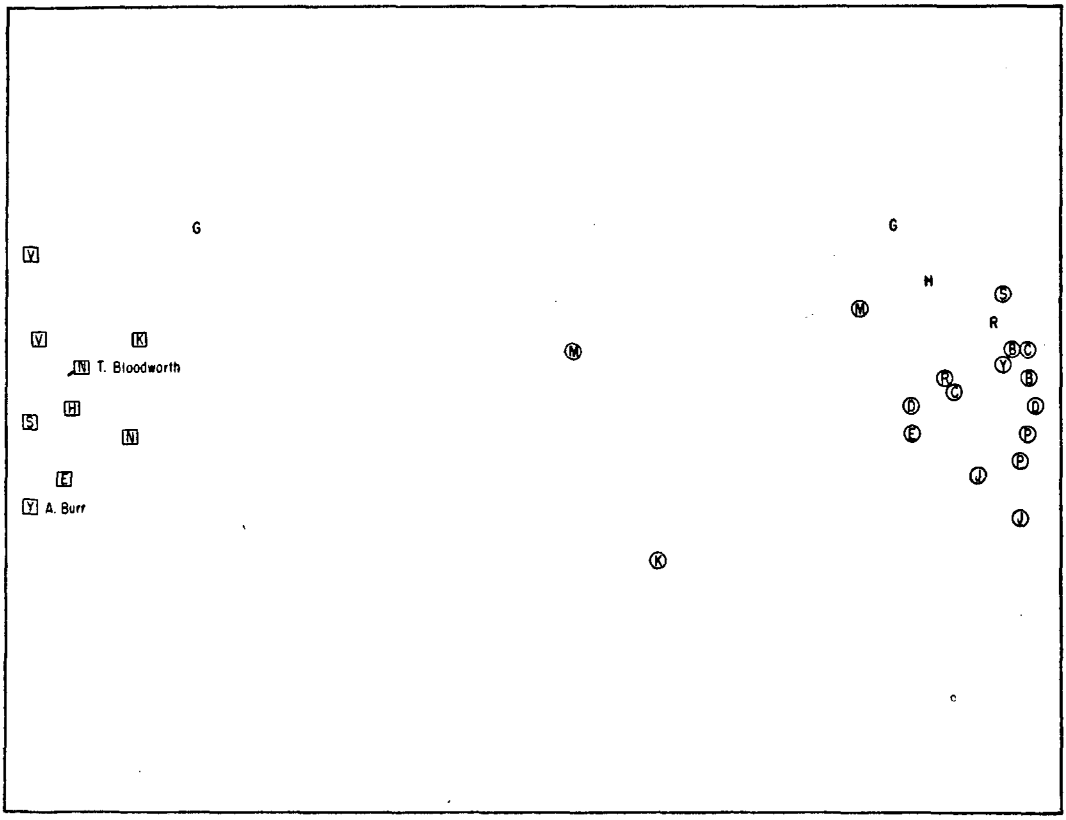
R = Rhode Island
S = South Carolina
T = Tennessee
V = Virginia
Y = New York

Ⓧ = Federalist
☒ = Republican
X = None/Unknown

Figure 7. Voting Patterns in the Second Congress, United States Senate, 1791–1793

by little political activity of any sort. Again, the candidates' political leanings were probably known to the more well-informed voters, but there was no open partisanship.

This lack of partisanship can be further illustrated by two individual cases. Elbridge Gerry, from the Middlesex district in Massachusetts, had been an Antifederalist in the Constitutional Convention, refusing even to sign the final document. He later became a leader in the Republican party, eventually serving as vice-president under James Madison. But in the Second Congress, Gerry voted more often with the Federalist bloc than with his nominal Republican allies. The second case is that of William Barry Grove, who had been elected in North Carolina as a Federalist supporter of the Constitution, defeating an incumbent who was



Source: Spatial configuration, derived from multidimensional scaling of recorded roll-call votes in the United States Congress (collected by the Inter-University Consortium for Political and Social Research).

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V = Virginia
Y = New York

Ⓧ = Federalist
☒ = Republican
X = None/Unknown

Figure 8. Voting Patterns in the Fourth Congress, United States Senate, 1795–1797

one of North Carolina's leading Antifederalists.[12] In spite of his Federalist label, his voting record placed him at the edge of the cluster of southerners, who mostly became known as Republicans. These two cases help to indicate that the patterns of voting in the Second Congress were not yet clearly determined by partisan affiliations.

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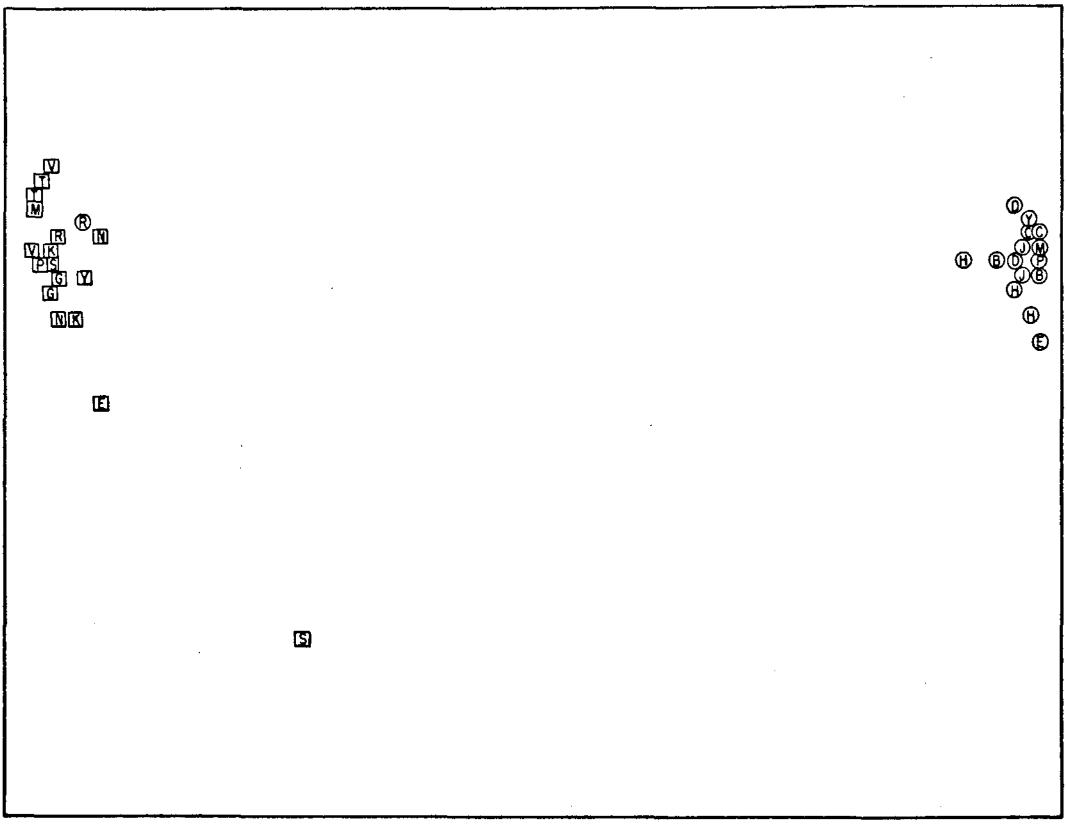
[12] Grove and his opponent, Bloodworth, were in sharp opposition over the wisdom of the Constitution, and the campaign was accented by a series of newspaper advertisements. In spite of an apparently heated contest where Grove won 65 percent of the 3166 votes cast, the election was very one-sided at the county level. In only two of the 12 counties in the district did the leading candidate get fewer than 90 percent of the votes, with each candidate winning those counties nearest to his home county. This "friends and neighbors" voting pattern is another clear sign of the lack of partisanship at this time (Gilpatrick, 1931; *North Carolina Chronicle*, 1791).



Source: Spatial configuration, derived from multidimensional scaling of recorded roll-call votes in the United States Congress (collected by the Inter-University Consortium for Political and Social Research).

Key: Each symbol indicates the state represented by a particular congressman. The circle or square drawn around the letter indicates party affiliation.

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P = Pennsylvania

R = Rhode Island
S = South Carolina
T = Tennessee
V = Virginia
Y = New York

Ⓧ = Federalist
☒ = Republican
X = None/Unknown

Figure 9. Voting Patterns in the Seventh Congress, United States Senate, 1801–1803

Polarization and Party Politics, 1793–1797

The polarization of congressmen into two political parties occurred during Washington's second administration, from 1793 to 1797. This process is clearly indicated by the lower levels of stress for the Third and Fourth Congresses, both in the House and the Senate (Table 1). But it is even more obvious from a comparison of configurations. There is a distinctly tighter clustering in configurations of the Third and Fourth Congresses (Figures 3 and 4, House; Figure 8, Senate), as compared to the First and Second Congresses (Figures 1 and 2, House; Figures 6 and 7, Senate).

In the Fourth Congress, the Senate configuration has a clearly bipolar structure (Figure 8), with two voting blocs corresponding to the emerging Federalist and Republican parties. The parties generally retained a highly sectional character, for the Republican bloc consisted almost entirely of southern senators, while the Federalist bloc included mostly senators from New England and the Middle States. Because of this overlap of party and region, there is some difficulty in determining whether these voting blocs formed because of partisan or regional affiliation. Nevertheless, for most cases where a senator's party ties and sectional loyalties were in conflict, he voted according to his party ties.

The configurations for the House of Representatives, like that for the Senate, show a clear movement toward a pattern of polarization (Figures 3 and 4). In each congress, two distinct polar groups form the cores of the two emerging parties. In addition, there are a number of individuals who do not clearly belong to either group. While this basic pattern appears for both the Third and Fourth Congresses, voting in the latter is more polarized. Instead of a large number of individuals who vote with neither cluster, nearly everyone in the Fourth Congress can be placed in one group or the other, although the groups are still not highly cohesive.

Something can be learned about the polarization process by a careful look at those congressmen who were not clustered with either polar group. In the Third Congress, over 20 of the 100 congressmen were in this intermediate position. Among these individuals, two particular categories are represented. The first type is the northern Republican (such as Gregg of Pennsylvania), who was moving toward his partisan allies from a position of regional loyalty. The second type is the southern Federalist (Parker of Virginia, Grove of North Carolina), moving in the opposite direction but again toward a position of partisan consistency. By the Fourth Congress, most northern Republicans had reached at least the fringe of the Republican bloc. The small group of southern Federalists was also loosely aligned with the Republicans, showing that partisanship was developing more slowly in the South.

Several general observations can be made about the progress of party development by the end of the Fourth Congress. First, the Federalists had developed quickly into a unified voting bloc, while cohesive voting emerged more slowly for the Republicans. This conclusion, supported by the spatial configurations, can be further confirmed by comparing the average agreement within the Federalist and Republican groups.

	Federalist	Republican
Third Congress	83.3%	73.5%
Fourth Congress	77.0%	72.7%

This difference might be partially attributed to the advantages of being in power. Because much of the agenda was set by the Federalist administration (with the active role of Hamilton), the Republicans in opposition had more difficulty reaching any kind of unity.

Second, both new parties retained a distinctive sectional character. In both the Third and Fourth Congresses, a majority of those in the Federalist bloc came from New England, and a majority of the Republican bloc were southerners. Given the great cultural and economic differences between regions, it should not be surprising that the parties were so distinctively regional.

Yet, in spite of the dominance of regionalism, partisan diversity had clearly emerged within several states. This was particularly true in the larger Middle States: New York, Pennsylvania, and Maryland. In each, elections were revolving around partisan concerns, and they were sending to Congress delegations which included partisans from both emerging groups. For the other regions at this time, partisan diversity was far more limited. New Hampshire, Vermont, and even Massachusetts (a center of Federalist strength) had elected a few men who voted together with the Republicans. South Carolina continued to provide virtually the only exceptions to Republican solidarity in the South. The sole remaining southerner to vote with the Federalist bloc was Richard Bland Lee, of Virginia, who was rewarded for his Federalist leanings with defeat in the election of 1795 (Cunningham, 1957). Most who identified themselves as Federalists in Virginia or North

Carolina still voted more frequently with their Republican neighbors.

By the end of the Fourth Congress, the American party system had reached the level of development labeled "polarization." Nearly every member of Congress could be classified with one party or the other, either by the political leaders of the time or on the basis of this analysis of voting agreement. Party organization and party discipline were growing, although still with limited effectiveness. Furthermore, political leaders such as Jefferson, Madison, and Giles were regularly discussing party activities in their correspondence (Cunningham, 1957). The issue which dominated the Fourth Congress and which several historians have regarded as crucial in party development was the contest over ratification and appropriation of funds for the Jay Treaty (Bell, 1973; Chambers, 1963; Charles, 1961). This treaty with Great Britain marked one of the first organized attempts by the opposition to defeat an important administration policy proposal. With Madison as party leader, Republican strategists tried to coordinate their opposition and defeat the treaty. In the end, they lost by a single vote, but their effort marked a critical point in partisan development (Cunningham, 1957).

The Emergence of Parties, 1797–1803

The Jay Treaty conflict (with its final vote on April 30, 1796) had a significant impact on subsequent congressional elections. These elections, held in late 1796 and early 1797, were dominated by the Jay Treaty issue and were characterized by a level of partisanship not previously seen. George Washington's retirement from politics, and the resulting choice between Adams and Jefferson for president, also tended to raise the level of partisanship. Thus these elections were important for the expansion of party politics into the electoral arena.

Spatial analysis of congressional voting in the Fifth, Sixth, and Seventh Congresses reveals a set of patterns which lend support to the idea that the 1796–97 elections began a new period of partisan politics. Voting in the House had become highly polarized and extremely well defined. The stress for these three configurations is very low and supports an excellent fit even in a single dimension (Table 1). The actual configurations (Figure 5) reveal two very tight clusters with only a few individuals outside of both blocs. In fact, the clustering is so dense that individual points in these figures cannot be labeled. For this reason, the partisan and regional distributions of the major voting blocs are provided in Table 2. In the Senate (Figure 9), the voting was also polarized into two groups, although the clusters are less well defined than in the House.

Cluster analysis of House members in these three congresses helps to demonstrate further the cohesiveness of the emerging parties. Average levels of agreement within the two voting blocs are shown below.

	Federalist	**Republican**
Fifth Congress	82.7%	86.9%
Sixth Congress	86.4%	86.0%
Seventh Congress	89.9%	79.6%

These levels of intra-group agreement are extremely high and far above comparable levels in previous years (or in fact, in modern times). The only real exceptions to the polarization into partisan voting blocs were in the Fifth Congress, where three individuals were noticeably unaligned: Dent (Md.), Parker (Va.), and S. Smith (Md.). All three served in Congress for several terms and had a history of inconsistent partisan ties. Yet, by the Sixth Congress, even these three men were clearly aligned with one of the party blocs.

There remains, of course, the question of whether these voting alignments can appropriately be called party blocs. This question can be examined by looking at the relationship between party labels and voting blocs, and by considering the sectional composition of these blocs. If voting blocs result from party alignments, they should be very consistent with party labels and relatively diverse in their geographical composition. The relationships in Table 2 are clearly consistent with the designation of the voting blocs as partisan.

At an individual level, this conclusion is illustrated by looking again at the voting record of Grove (N.C.). He was first elected to Congress in 1791 and was always regarded in his home state as a Federalist, the only North Carolina Federalist chosen before 1798. Yet in the Second, Third, and Fourth Congresses, his voting record was more in line with the Republican bloc and the rest of his state delegation. By the Fifth Congress his votes placed him on the edge of the Federalist bloc; and in the Sixth and Seventh Congresses, he was clearly a partisan Federalist. Furthermore, in the latter two congresses, Grove was joined in the Federalist camp by several other North Carolina Federalists. A second southern Federalist, Parker (Va.), followed a similar path, moving from his sectional group to his party affiliation.

The issue which seems to symbolize the emergent partisanship was the passage of the Alien and Sedition Acts in 1798. These acts were at least partially an attempt by the Federalists to destroy the opposition party. Votes on these laws coincided almost perfectly with the blocs shown in the configurations, providing further evidence that these blocs were partisan groupings.

Indices of Partisanship

A more traditional type of roll-call analysis may help to amplify further the multidimensional scaling results. Three indices of party voting are employed here—Rice's index of cohesion, Rice's index of difference, and Lowell's concept of the party vote. The utility of these indices for studying partisanship across time has been demonstrated by MacRae (1970, pp. 200–07), who showed that the degree of partisanship in Congress (as measured by party votes) rose to its highest levels immediately following party realignments. Because the emergence of new parties might be regarded as a special case of partisan realignment, a similar pattern might be expected between 1789 and 1803.

The values of the index of cohesion, the index of difference, and the percentage of party votes are presented for the House and Senate from 1789 to 1803 in Figure 10. These data readily show that cohesion in both legislative parties increased dramatically during this period, while party differences grew simultaneously. The indices of party cohesion and difference rose constantly during the first five congresses, leveling off after 1797. The timing of these changes corresponds rather closely with the polarization observed in the MDS configurations. Parties developed steadily from 1789 until 1796, the year of the Jay Treaty controversy. By the Fifth Congress, they had reached a high level of development on any measure of congressional voting, a level sustained through 1803.

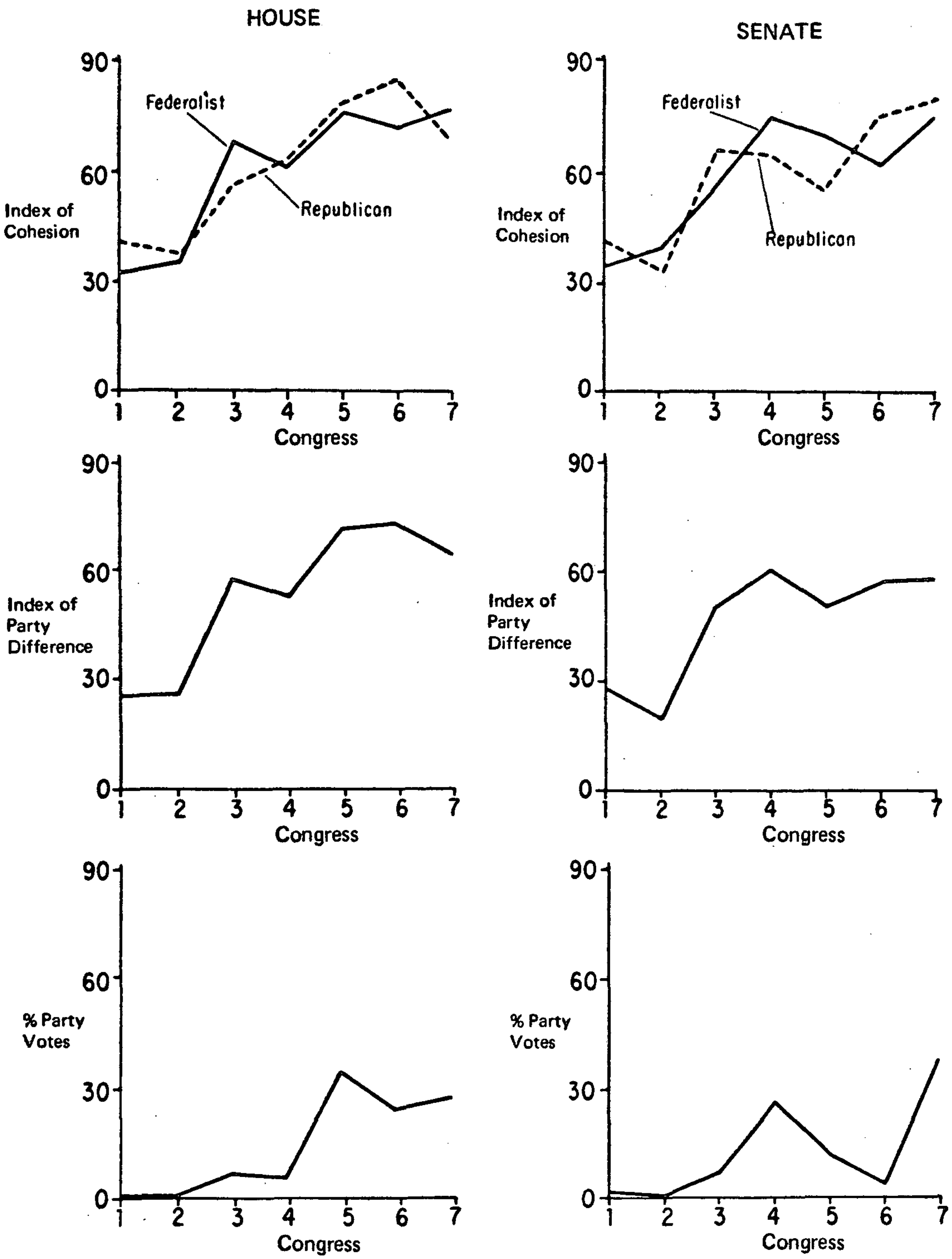
For purposes of comparison, I have also calculated indices of cohesion and difference for the three regional groups: New England, Middle States, and the South. Regional cohesion remained reasonably constant across this period, although some changes did occur in the Sixth and Seventh Congresses, reflecting large partisan swings within several states in the elections of 1798 and 1800. The average regional cohesion for the entire period was 49.3, a level higher than party cohesion in the first two congresses but exceeded by party cohesion in all the remaining years. This result confirms the earlier conclusion that partisanship had become more important than sectionalism after about 1793.

In order to make a better assessment of party development in these early years, comparisons can be made between the cohesion of the early parties and twentieth-century parties. During the entire period from 1921 to 1969, the average index of cohesion of the two

Table 2. Partisan and Regional Distributions of Members of the United States House of Representatives, by Multidimensional Scaling Blocs

Multidimensional Scaling Bloc	Total	Party: Federalist	Party: Republican	Party: None	Region: New England	Region: Middle States	Region: South	Region: West
Fifth Congress								
Federalist	52	51	1	0	25	21	6	0
Republican	45	0	44	1	3	12	27	3
None	3	2	1	0	0	2	1	0
Sixth Congress								
Federalist	51	51	0	0	22	15	14	0
Republican	49	6	43	0	3	21	22	3
Seventh Congress								
Federalist	38	37	1	0	20	10	8	0
Republican	62	3	59	0	8	26	25	3

Source: Derived from multidimensional scaling of recorded roll-call votes in the United States Congress (collected by the Inter-University Consortium for Political and Social Research).



Source: Computed from recorded roll-call votes in the United States Congress (collected by the Inter-University Consortium for Political and Social Research).

Figure 10. Trends in Partisanship, United States Congress, House of Representatives and Senate, 1789–1803

congressional parties was about 66, ranging from 55 to 77.[13] According to this standard of comparison, 1795 seems to mark the year when early parties reached the level of unity found in the twentieth century. Furthermore, beginning in the Fifth Congress, the level of party cohesion exceeded that in nearly any modern congress. Similarly, the average index of party difference for the twentieth-century congresses was about 41, a figure surpassed by the time of the Third Congress. The number of party votes averaged about 10 percent of all votes between 1921 and 1969; again, the early parties generally matched these levels. In fact, the proportion of party votes after 1797 had reached a level obtained at few times in American history.

Discussion and Conclusions

The loose patterns of association known as factionalism had been present in nearly every colonial and national assembly predating the Constitution, and these patterns continued in the early years of Congress. For the First House, evidence of such patterns existed in the regional voting blocs. At least minimal patterns of association characteristic of factionalism can be detected for every congress, with the exception of the unstructured voting of the First Senate. Indeed, it is difficult to imagine a legislative body without factions, particularly in a new nation with considerable geographic, economic, and cultural diversity.

While it seems natural that factions should have arisen, it was not inevitable that these factions should have polarized. Yet the empirical evidence shows that such a movement occurred. Examination of the chronological sequence of configurations, for either the House or the Senate, reveals a vivid picture of increasing polarization, culminating in the emergence of two highly cohesive voting blocs by 1797. These voting blocs were in fact more cohesive than twentieth-century parties.

Three important questions can be raised regarding this polarization of legislative voting: Why did particular alignments develop, or in fact why did any alignments appear at all? Second, should these polarized voting blocs be identified as parties? Finally, how can we explain the anomalous development of parties (if that is indeed an appropriate term) during a period when most political leaders were immersed in a strong anti-party philosophical tradition?

Reasons for the Emerging Alignments. Huntington (1968, p. 415) argues that polarization is likely to be triggered by either "the cumulation of cleavages" or "the emergence of a single dominant issue which overshadows all others." In the American case he suggests that the fiscal program presented by Hamilton to the First Congress met this latter criterion. The evidence presented here, however, does not support this claim, for the factions did not polarize for several years. This issue was merely a precursor of the new alignments, particularly in terms of the leadership of Madison and his fellow Virginians. Others have suggested that the Jay Treaty provided the polarizing force. A more plausible case, however, can be made for the idea that polarization resulted from the cumulative effect of several issues, all of which came together in the Jay Treaty conflict.

Several cleavages were generally important in the early years of the new republic. One was the sectional division between North and South, which was manifested on issues such as the location of the capital for reasons of simple regional loyalty. A second significant conflict was that between commercial and agrarian interests (Beard, 1915), economic differences intensified by their convergence with the regional divisions. The northern economy was built upon commercial interests (shipping, fishing, trade, banking, and manufacturing), while the southern economy (with exception of coastal South Carolina) was more agrarian, with an abundance of plantations dependent on slavery (Dauer, 1953). A third cleavage existed between coastal and interior regions, the former naturally more concerned with trade and other commercial interests, and the latter, with agrarian economies, being concerned with internal improvements and protection of the frontier. A fourth cleavage set those wishing to maintain close relations with England against those more sympathetic to France. Against the background of the French Revolution, this division was a manifestation of a deeper philosophical disagreement over the form of democracy desired in this country, a dispute which provided the fifth major cleavage.

All five divisions were present in the political arena of 1795, and each tended to reinforce the emerging alignment of Federalists and Republicans. Federalists, coming most often from New England or the coastal regions of the Middle States and South Carolina, were generally representatives of commercial interests, sympathizers with the English, and supporters of a more aristocratic political system. Republicans, on the other hand, came generally from the interior or the South, and they were mostly allies of agrarian interests and believers in the direct democracy symbolized by the French Revolution.

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[13] The averages for the congresses of the twentieth century were computed from tables in Cooper, Brady, and Hurley (1977, pp. 138–39).

At a time when these blocs of interests were coalescing, controversy arose over ratification and implementation of the Jay Treaty. The treaty itself was the outcome of negotiations with Great Britain over a number of concerns, including British activity on the western frontier, British seizure of slaves during the war, American trade debts dating to before the war, the neutrality of American shipping in the war between Britain and France, and the general status of British-American trade. As the product of the Federalist administration, the treaty was opposed by the Republicans, who were particularly concerned about the favorable trade status granted to Britain at the expense of France and the failure to provide compensation for slaves seized by Britain (Chambers, 1963; Charles, 1961; Varg, 1963). The Jay Treaty, therefore, represented in a single issue the cumulation of dominant cleavages in early American politics. The subsequent polarization of congressional voting and increased partisanship in electoral contests made this issue a crucial factor in the development of American political parties.[14]

The Existence of Party. While there should be little question about the polarization of alignments in the early American congresses, reasonable objections can be raised to the designation of polar voting blocs as parties. Formisano (1974), for example, has suggested that voting blocs may have formed not along party lines, but rather along lines related to such factors as regionalism and boardinghouse residences. Obviously, region and party were highly related during this period, a fact which itself does not run counter to the existence of parties. But the multiplicity of cleavages and the persistence of voting alliances would seem to deny a simply regional explanation. In fact, by the end of this era, the voting blocs had become less dominated by a single region (see Table 2). And, as long as the capital remained in New York or Philadelphia, boardinghouses were not a salient factor in congressional voting. Furthermore, when Congress did move to Washington in 1800, there is no evidence of distinct voting blocs corresponding to boardinghouse membership. Indeed, it is more likely that congressmen chose as messmates their established partisan allies.[15]

Nevertheless, there is more to the idea of party than simply the existence of congressional voting blocs. The several aspects of party identified early in this article include common symbols or labels and groups of supporters in the electorate. Formisano (1974, p. 478) has pointed out that

> multivariate analysis of roll calls needs to be accompanied by evidence that men ran for Congress as openly identified members of a party. To show convincingly that the core factions of Federalist and Republican interests came to think of themselves in party terms, consideration must be given to legislators' self-images. . . .

Although there was not a consistent use of party labels in election campaigns across the entire nation, it is clear that party labels were becoming more meaningful as legislators polarized into voting blocs. Gradually people were beginning to think about politics in explicitly partisan terms (Cunningham, 1957; Young, 1967).

The determination of whether parties existed in the 1790s has become in effect a game of definitions. Evidently, this was a decade of party development, and whether the word "party" is appropriate at any particular time is not crucial. What is important is to document the continually increasing prominence of parties in the politics of this era. In 1790, members of Congress were voting together in regional groups whose composition shifted considerably from one issue to the next. By 1792, patterns of voting were shifting from a regional basis to a more partisan basis built on a set of issue positions. By 1796, these voting patterns had polarized so that two blocs of congressmen opposed each other on nearly every issue. As the cohesiveness of party groups in Congress increased, party affiliation became a more significant factor in congressional elections. These trends continued until at least 1803. Only thereafter, with the decline of the Federalists, did the country experience a temporary hiatus in the process of party development.

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[14] For further discussion, see Hoadley (1979).

[15] The boardinghouse explanation of voting cohesion was advanced by Young (1966), but his conclusions have been recently challenged (Bogue and Marlaire, 1975; Cunningham, 1978). A comparison of boardinghouse residence (found in Goldman and Young, 1973) with the location of members in the configurations for the Philadelphia and Washington years (to 1803) fails to reveal any pattern supporting Young's thesis for this period.

Parties in an Anti-Party Era. When the new government was created by the Constitution, most political leaders shared a strong anti-party tradition. Parties were perceived as agencies which would hinder the progress of good government, a view expressed forcefully by Madison in *The Federalist,* No. 10 (1961, p. 77): "Among the numerous advantages promised by a well constructed Union, none deserves to be more accurately developed than its tendency to break and control the violence of faction." Yet in spite of such sentiments, parties very quickly became a prominent feature of the political system. This anti-party stigma does, however, help to account for the fact that parties had not reached the highest stages of development during this period. The reluctance of candidates to run openly for office as members of a party and the failure of leaders to develop any substantial national organization were two factors that might be explained in part by this distrust of the idea of a party system.

One explanation for the anomaly of party development within the context of an anti-party tradition lies in the lack of any positive historical precedent. Nowhere in the eighteenth-century world did parties exist by any modern definition. There was considerable factionalism, both in Great Britain and in colonial America, but no clear development of parties beyond this stage. Therefore, in their anti-party statements, American political thinkers were dealing with a concept of "party" which today would be characterized as "faction." They simply did not recognize the possibility of a more constructive political party until some time after they had actually created such parties.

A second explanation for this anomaly lies in the emerging idea of legitimate opposition. Some of the philosophical objections to the idea of party were based on the belief that parties might hinder development of the unanimity considered essential for the stability of a state. Gradually people realized that unanimity was impossible in a diverse society and that the right of opposition had to be recognized. Sartori (1976, p. 11) has suggested that "parties presuppose—for their acceptance and proper functioning—peace under a constitutional rule." The first set of elections for Congress (between 1788 and 1790) showed that the Constitution was quickly being accepted. In spite of the large numbers opposing ratification in some states, most of the new congressmen were supporters of the Constitution. Furthermore, even an Antifederalist such as Elbridge Gerry could be found arguing in debate that, "If this constitution, which is now ratified, be not supported, I despair of ever having a government for these United States" (Austin, 1829, p. 103).

When it became clear that the opposition was concerned only with specific policy alternatives within the existing constitutional system, it was far easier for such opposition to be tolerated. Once the legitimacy of opposition was established, the acceptance of parties followed. This was, of course, a slow process. The Sedition Act, passed by Federalist majorities in 1798, represented the old tradition; at least one Republican congressman (Lyon of Vermont) won reelection after being convicted and jailed under this act. Nevertheless, the progress of party development was not deterred seriously by this act of a declining Federalist majority. Only two years later, the Republicans were victorious in both presidential and congressional elections. That a transfer of power occurred peacefully in 1801 reflects growing acceptance of the legitimacy of opposition and the idea of party.

In 1804, Thomas Jefferson wrote in a letter, "The party division in this country is certainly not among it's pleasant features. To a certain degree it will always exist..." (quoted in Cunningham, 1965, p. 19). By this time, nearly everyone in Congress was clearly associated with one of the new parties. Acceptance of these parties, however, came reluctantly as their inevitability became more apparent. Whether recognized by those involved or still regarded as an undesirable development, the changing voting patterns exhibited by members of Congress between 1789 and 1803 present strong evidence that a true party system had emerged.

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COMMUNICATIONS

Comments on Sullivan, Piereson, and Marcus (Vol. 73, September 1979, pp. 781–94)

TO THE EDITOR:

In recent years several social scientists have concluded that the tolerance of the American electorate toward ideological nonconformists has increased (Nunn, Crockett, and Williams, 1978; Davis, 1975). The recent article by John L. Sullivan, James Piereson, and George E. Marcus (1979) calls this conclusion into question. While tolerance toward such groups as communists, atheists, and socialists has increased, they argue that this may simply be because these groups are less salient today than they were in the mid-1950s when Stouffer's classic study was conducted (1955).

Sullivan and his colleagues have made a major substantive and methodological contribution to the study of tolerance. Substantively, they strongly suggest that levels of tolerance among the American electorate may still be fairly low. At a methodological level, they demonstrate that one can develop a measure of tolerance that allows each respondent to express his or her tolerance to a specific group that the respondent dislikes. Once such measures are used, tolerance is often revealed to be very low. Sullivan and his colleagues compare the results of their "content-controlled" items with items about communists and atheists taken directly from the original Stouffer questionnaire. When one examines levels of tolerance in 1976 (a Twin Cities study) or in 1977–1978 (NORC surveys), one finds that respondents are less tolerant toward their least-liked group (the "content-controlled" measures) than they are to either communists or atheists. Differences are greatest on a question about whether members of these groups should be allowed to teach. Focusing largely on a comparison of the questions about teaching, Sullivan and his colleagues conclude, "Certainly this suggests that although tolerance of communists and atheists has increased, the overall extent of tolerance may not have changed much at all" (p. 788).

Sullivan and his colleagues may have overstated their case when they argue that tolerance has not increased much at all during the last two decades. Their case rests mainly on evidence that, as of 1976 and as of 1977–1978, tolerance was lower with their "content-controlled" questions than with the Stouffer questions about communists and atheists. But although their content-controlled measure is quite interesting, it is difficult to attach much substantive meaning to these results. That most Americans would deny civil liberties to the group they like least may or may not pose a threat to civil liberties. As it happens, the targets of intolerance are widely diffused, a condition that Sullivan et al. acknowledge prevents intolerance from threatening civil liberties at the present time.

Assuming that one can meaningfully compare levels of tolerance for specific groups with the "content-controlled" measures, Sullivan and his colleagues tend to ignore differences in the wording of questions about least-liked groups and the Stouffer questions about communists and atheists. In fact, of the six content-controlled questions, only two have any similarity with the Stouffer questions: those about making a speech and about teaching.

For the freedom of speech question there was actually no difference between the percentage in the Twin Cities study that would allow a communist to speak and the percentage that would allow a member of their least-liked group to speak, although there were significant differences in the NORC study. The teaching questions were substantially different in their wording. Respondents were asked whether members of their "least-liked" group "should be allowed to teach in public schools," while they were asked whether a person who was against all churches and religion should "be allowed to teach in a college or university." As for an "admitted communist," respondents were asked, "Suppose he is teaching in a college. Should he be fired, or not?" A "public school," I suspect, does not conjure up the image of the University of Minnesota, but of elementary schools, middle schools, and high schools. Some persons might think a communist should not be allowed to teach, but might feel that actually firing a college teacher was too drastic. I do not know whether such differences in wording have any effect, and, in Sullivan's defense, it should be noted that in 1954 respondents did not differ as to whether a person "whose loyalty has been questioned" but "who swears under oath he has never been a communist" should be fired from a college or from a high school. (Nearly seven out of ten thought he should not be fired.) However, Sullivan has shown that in other contexts seemingly minor differences in question wording can have a dramatic effect on the results (see Sullivan and Minns, 1976; Sullivan, Piereson, and Marcus, 1978), so a little more caution here would be in order.

If we assume that there is comparability between the "content-controlled" questions and the Stouffer items (or if we merely assume that differences of wording do not bias the Stouffer questions to *deflate* tolerance), the very data presented by Sullivan and his colleagues suggest that tolerance may have increased between the mid-1950s and the late 1970s. For example, in his 1954 survey Stouffer found that only 27 percent of his cross-sectional sample would allow a communist to speak in their communities, and only 37 percent would allow an atheist to speak. In 1976, 70 percent of the Twin Cities survey, and in 1978, 50 percent of the NORC sample would allow a member of their "least-liked" group to speak. In 1954, only 6 percent thought a communist should be allowed to teach in a college, and only 12 percent thought an atheist should be allowed to teach at a college or university. In 1976, 26 percent of the Twin Cities sample would allow a member of their "least-liked" group to teach in a public school, and in the 1978 NORC sample, 19 percent would. Of course, we do not know how many respondents in 1954 would have allowed a member of their "least-liked" group to speak or to teach. As communists could hardly have been everyone's least-liked group, tolerance might have been even lower (although it's hard to get below a figure of 6 percent tolerant). But it does appear that respondents were more likely to be tolerant of their least-liked group in the 1970s than they were toward communists or atheists back in the mid-1950s.

Certainly, tolerance may still be fairly low. Certainly, we would be well advised to accept some of the cautions that Sullivan et al. have raised. But I do not think we need to accept their pessimistic conclusion that tolerance may not have increased much at all during the last two decades. Indeed, the very data they present provide at least some evidence that tolerance has increased.

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TO THE EDITOR:

In a recent article in the *Review*, and also in a popular presentation in *Psychology Today*, John L. Sullivan, James E. Piereson, and George E. Marcus shed new light on the problem of political tolerance. A number of recent studies have claimed to find increases in tolerance since the low levels noted by Stouffer in the 1950s, but Sullivan and his colleagues raise serious problems for this entire research tradition.

Their study makes two major points, one methodological and one substantive:

1. *The methodological thesis.* The standard measurements of tolerance are flawed by a serious methodological problem; as a result the studies that show increases in tolerance are invalid.
2. *The substantive thesis.* Although the levels of tolerance for particular groups such as communists have increased, overall levels of tolerance have not increased significantly since the 1950s. "The mass public," according to the authors, "is still generally intolerant today" (1979, p. 789). "Intolerance has by no means disappeared; it has simply shifted targets" (*Psychology Today*, p. 87).

In what follows I will argue that Sullivan and his associates are on much firmer ground with the methodological thesis than they are with the substantive thesis.

The substantive thesis does not follow directly from the methodological thesis. The fact that an argument is invalid does not mean that its conclusion is false. Someone might present an invalid argument to show that the sun is the center of the solar system. Although the argument would be invalid the conclusion is still true.

In the same way, Nunn and others may have used invalid arguments to show that Americans are becoming more tolerant (Nunn et al., 1978). The fact that those arguments are invalid does not show that levels of tolerance remain unchanged. It is perfectly possible that tolerance has increased, even though the methods used to prove this are fallacious.

In order to see why Sullivan and his associates believe that tolerance has not changed, it is necessary to review the details of their argument. Previous studies of political tolerance have used a measurement strategy that might be described as "content-bound." In this approach the respondent is asked about specific actions performed by members of unpopular groups, the groups being chosen by the researcher. Stouffer, for example, asked questions such as whether communists should be allowed to teach in public schools.

Their insight is that such questions measure two different things—general levels of intolerance and attitudes towards specific groups. The discovery that these content-bound questions are ambiguous raises serious problems for those who use them to measure changing levels of tolerance. The fact that more people are willing to tolerate communists today than 20 years ago does not necessarily prove that people are more tolerant. It may only prove that people are now less concerned about communists.

Sullivan and his associates use an alternative measurement that employs "content-controlled" questions. They first ask the respondent to select his least-liked group. The respondent is then asked questions concerning toleration of members of that least-liked group. The theory is that the respondent's willingness to tolerate members of his least-liked group is a truer reading of tolerance than questions concerning tolerance for members of groups selected by the investigator.

Sullivan and his associates thus provide a devastating critique of the studies that show increases in the level of tolerance, and they also provide a promising new way to measure tolerance. None of what has been said so far, however, establishes the substantive conclusion that levels of tolerance have not changed since the 1950s.

In order to say that levels of tolerance have remained constant over the last 20 years, one must have at least two points of reference for comparison. Sullivan and his colleagues derive their finding of lack of change by comparing the responses given to Stouffer's questions in the 1950s with the responses given to their own content-controlled questions in the 1970s.

What does this comparison prove? The finding that Americans are just as intolerant as they were 20 years ago is based on a comparison of content-bound questions asked during the 1950s with content-controlled questions asked in the 1970s. But content-bound questions and content-controlled questions do not, as the authors have shown so convincingly, measure the same thing. How then can they be used as the basis of comparison?

The problem is that there is a tension between the methodological thesis and the substantive thesis. When Sullivan and his colleagues are discussing methods of measuring tolerance, they are quick to discredit the technique used by Stouffer. The authors write that Stouffer's method of measuring tolerance "is inadequate because it does not fully capture the meaning of the concept of tolerance. It is time-bound because it presumes that these particular groups are the only important targets of intolerance in the society" (1979, p. 792). In order to make their methodological point, then, they reject Stouffer's findings.

When the authors come to establishing their substantive conclusion, however, they rely on the Stouffer study. The thesis that levels of intolerance have not changed since the 1950s follows only if one is willing to take the Stouffer study as a benchmark. But in taking Stouffer's results as a benchmark, Sullivan and his colleagues commit themselves to the validity of content-bound measurements of tolerance, the very mistake that they condemn in others. The comparison only works if one assumes that Stouffer's tests were an accurate measurement of tolerance in the 1950s. Sullivan and his colleagues give no evidence for believing this, and their general critique of content-bound measurements gives us strong reasons to disbelieve it.

Logically speaking there is a world of difference between "earlier studies fail to establish increases in tolerance," and "there are no increases in tolerance." The article is very persuasive in making a case for the former, but much less convincing in its argument for the latter. In fact, Sullivan and his colleagues are usually rather cautious in how they state the substantive thesis. In most places they limit themselves to defensible statements such as: "Intolerance has not necessarily declined much over the past 25 years" (p. 792). At other times, however, they formulate the substantive thesis in ways that are technically correct but easily misinterpreted even by an educated reader. Compare for example the technically correct formulation of the thesis given by the authors in the *Psychology Today* article with the headline for the article (written presumably by the magazine's editor):

> *From the article:* "When people are asked specifically about the groups they dislike most . . . they prove to be just as intolerant as they were during the 1950s toward Communists and atheists."
>
> *From the headline:* "A new study asserts that people are just as intolerant today as they were in the early 1950s. Only the targets have changed" (*Psychology Today*, p. 87).

The first statement if supported by the evidence but the second is not. There is a very fine line between what Sullivan and his associates have proved and what they have not proved. Because they themselves are often not clear about it, it is easy to see how others could become confused as well.

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Reply

TO THE EDITOR:

We are indebted to John Immerwahr for taking the time to read our article carefully and to think seriously about its conclusions. Though his comments are fair and judicious, we cannot agree with everything he says.

His objections to our article may be summarized as follows: Though our methodological critique of the earlier literature is sound, as is our measure of tolerance, our substantive thesis that levels of tolerance have not increased in the United States since the 1950s is not demonstrated by our analysis. Since the Stouffer measures are rejected as unreliable, we have no standard by which to compare current levels of tolerance with those that might have existed in the 1950s. As a result, we cannot prove conclusively that levels of tolerance have not increased during this period; the most we can show is that others have not affirmatively proven that levels of tolerance have increased. There is a world of difference, he suggests, between the claims that "earlier studies fail to establish increases in tolerance" and "there are no increases in tolerance." Our article makes a strong case for the former, but is much less convincing in relation to the latter. Thus, the title of our paper, in which we describe these reputed changes as "illusory," is inaccurate and misleading.

What exactly did we say in our article? After showing that existing measures of tolerance are inadequate, we proceeded to develop our own measure and to compare the results derived from this measure with those derived from the Stouffer measures. We showed beyond a reasonable doubt that the Stouffer measures, because of their focus on left-wing groups, exaggerate the real level of tolerance as measured by our "content-controlled" procedure. We therefore inferred, quite safely, we think, that studies based on recent replications of the Stouffer items overstate the amount of real change that has occurred in levels of tolerance since the 1950s. Since the current level of tolerance is not as high as these studies suggest, it is hard to see how things could have changed as much as their authors assert.

The most problematic question here is, how much have things really changed? We suggest in the article that levels of tolerance have not increased much since the 1950s, or may not have increased very much, or have not necessarily increased much. This is where Immerwahr leaves our company, because he says we have no evidence whatever to support this claim. The truth, therefore, seems to lie somewhere between the ranges of "tolerance has not increased since the 1950s" and "tolerance has not increased as much as current writers claim." It is now impossible to determine precisely how much things have changed, since the Stouffer measures are the only ones that we have from the earlier era, and these have been rejected as invalid. Thus, any evidence bearing on the question must be to some degree inconclusive. But if the evidence for our case is not fully conclusive, neither is it for the alternative view that levels of tolerance have increased greatly since the 1950s. It is thus unfair to impose the entire burden of proof on us, since those who disagree with our conclusion are obliged to provide some affirmative evidence that levels of tolerance truly have increased—that is, if they find our methodological argument persuasive.

But since we advanced the position, we are perhaps obliged to defend our terrain.

There is, logically speaking, a world of difference between the claims that "there is no evidence whatever for an argument" and "the evidence on which an argument rests is not conclusive." Immerwahr describes our conclusion in terms of the former, when in truth it is more accurately characterized in terms of the latter. He does so because he believes, incorrectly, that we use the earlier Stouffer results as standards against which to compare current levels of tolerance as revealed through our content-controlled procedure. When this tactic falls in the face of our own methodological critique, he asserts that we are left without any evidence to support our claims. We do not say, and cannot say, given our criticisms, that the Stouffer items constitute reliable measures of tolerance either for the 1950s or the 1970s. We do suggest, however, that these items were more appropriate in 1954, when attention was focused on the standard left-wing groups, than they are today, when the potential targets of intolerance are more numerous and ideologically diverse. Thus, we suspect, but cannot prove, that the disparity in results reported in our article between our content-controlled procedure and the Stouffer items would have been much smaller had the same experiment been performed in 1954. In addition, given the low level of tolerance reported in his original study, it is hard to see how Stouffer could have overestimated the real level of tolerance in 1954 by any great amount. The Stouffer items are invalid on their face, but the passage of time has rendered them even more inappropriate than they once were. Paradoxically, the very transformations that the measures seek to track have served to distort and to undermine them. The very processes that have made people more tolerant of communists, socialists, and atheists have also rendered these groups less important as political threats.

But in any event our conclusion does not rest on this chain of reasoning alone. Our empirical analysis showed (1) that the Stouffer items overstate the real level of tolerance and (2) that current levels of tolerance are still very low. Thus, if levels of tolerance have increased since the 1950s, they could not have increased very much, and certainly not as much as some have claimed. In addition, we have suggested elsewhere (1979; see citation, n. 2) that the changes reflected in the Stouffer items between 1954 and 1974 were due primarily to the declining salience of the three groups mentioned in the questions (communists, socialists, and atheists) rather than to any significant change in attitudes toward the *acts* mentioned in the questions (allowing members of these groups to speak, teach, or to place their books in libraries). To be sure, none of this conclusively proves our case. But since conclusive proof is not forthcoming, a judgment must be made by weighing this evidence against that presented by those who claim that great changes have occurred. When the evidence is so weighed, it appears to us sufficient to justify the conclusions reached both in our longer article and in its popular presentation.

It is worth noting that Immerwahr does not seem to disagree so much with what *we* say as with what others might think we say. He notes in his concluding paragraph that we are rather cautious in the way we state our substantive thesis, though he then goes on to say that we are not clear about what we have proved and what we have not proved. Despite our caution, or perhaps because of it, he suggests, some may misinterpret what we say. We would have been less cautious if our problem had admitted of a definitive solution; since it did not, our caution seems to have been fully justified.

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Reference: Sullivan, John L., James Piereson, George E. Marcus, and Stanley Feldman (1979). "The More Things Change the More They Stay the Same: The Stability of Mass Belief Systems." *American Journal of Political Science* 23: 176–86.

Comment on Ostrom (Vol. 72, September 1978, 941–57)

TO THE EDITOR:

Charles W. Ostrom's article, "A Reactive Linkage Model of the U.S. Defense Expenditure Policy-Making Process" (1978) laudably attempts to synthesize competing models of the defense spending process through the use of a series of linear equations estimated by restricted least squares. He uses two major criteria to judge the success of his endeavor. First, he examines the parameter estimates for the model's regression equations, which he finds to be both "sharp and plausible" (p. 955). Second, Ostrom examines the predictive capability of his model and maintains that the "model as a whole generates extremely accurate historical forecasts" (p. 941). Unfortunately, the criteria used to assess the model are artificial because they lack useful real world meaning. His article is thus an instance of the investigator giving undue deference to standard statistical measures without asking what they mean in substantive terms.

First, consider the predictive accuracy of Ostrom's model. Ostrom judges his model to provide extremely accurate estimates of defense spending by comparing its estimates to the actual data. He does the same with a naive model. The latter model says that this year's expenditures are the same as last year's. He then compares the root mean square error (RMSE) of the estimates from the two models and asserts that his model is superior to the naive model because his model's RMSE is $4 billion while the RMSE for the naive model is $5 billion. For a number of reasons, a RMSE of $4 billion is too large, especially when the total expenditures rante from only $35 billion to $77 billion. If government spending is viewed as a fiscal tool, one is forced to admit that a $5 billion error would make a difference in the impact of defense spending. More importantly, if defense spending is viewed in programmatic terms, errors on the order of $4 billion are unacceptable. Entire weapons systems can cost less. To the political scientist who wishes to maintain some credibility with a nonacademic, policy-oriented community, predictions with an error so large can only bring derision.

Table 1. Rank Ordering of Prediction Errors Arising from Ostrom's Reactive Linkage Model and a Naive Model

	Absolute Value of Errors (in Million Dollars)	
Rank	Reactive-Linkage Model	Naive Model
1		213
2		258
3	398	
4		462
5		499
6		625
7		722
8	971	
9	1038	
10	1268	
11		1351
12	1486	
13	1513	
14		1604
15	1623	
16	1767	
17	1781	
18		2012
19	2076	
20		2358
21		2647
22	2710	
23	3013	
24		3158
25	3535	
26		3588
27	3658	
28	3707	
29		3787
30		4510
31		4803
32	5881	
33	5985	
34	7372	
35		8205
36	9452	
37		9826
38		13369

Source: Compiled from Charles W. Ostrom, Jr., "A Reactive Linkage Model of the U.S. Defense Expenditure Policymaking Process," *American Political Science Review* 72: 953, Table 4.

Perhaps more disturbing to those who value methodological rigor is the fact that Ostrom's analysis of the predictions is incomplete. Relying on the aggregate-level RMSE glosses over subtleties that are essential to examining carefully the goodness of fit of the reactive linkages model. In fact, in only 8 of the 19 years being studied is the error of the reactive linkage model smaller than the error of the naive model. In other words, in the *majority* of cases the naive model more closely approximates the actual expenditure data.

Since the naive model is more frequently closer to actual expenditures than the reactive linkage model, it is important to examine the distribution of errors stemming from each model. It is possible that the distribution of errors from the two models are not substantially different. To test that hypothesis, I rank-ordered the absolute values of the errors from both models (see Table 1). As suspected, there is great commingling in the ranks of the errors of the two models. Applying a Kruskal-Wallis analysis of variance to the ranks yields an H or chi square of .109. That statistic is significant at the .7 level. Combining that result with the fact that the naive model more frequently makes a closer estimate, it is highly likely that the naive model predicts as well as the reactive linkage model. The standard of parsimony would lead most analysts, therefore, to accept as superior the naive model's prediction that this year's expenditure is the same as last year's.

Returning to Ostrom's first criterion for assessing his model highlights another problem.

How does the analyst account for the disparity between the poor fit of the reactive linkage model in substantive terms and the "sharp and plausible" estimates of the parameters in the regression equations? Even if the coefficients in the regression equations are sharp by commonly accepted statistical standards, what do they mean if the model fits the data so poorly?

Ostrom should be complimented for his efforts to integrate competing models, but his model is not superior to a very naive and unacceptable model. It is not enough for scholars to evaluate their research solely according to "academic" standards. The interpretability of their findings in real-world terms must also be considered.

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Reply

TO THE EDITOR:

John Wanat's communication regarding the Reactive Linkage Model asserts (1) the absolute magnitude of the forecasting errors is too large for the model to be useful to policy makers; (2) the model is, contrary to my findings, less accurate than a simple naive model; and (3) the import of the sharpness and plausibility of the parameter estimates, to a policy analyst, must be questioned in light of the poor performance of the model. As a consequence of these assertions, he suggests that the policy community has no choice but to view the Reactive Linkage Model with "derision." Accepting Wanat's conclusions, we are left with the "parsimonious" no-change naive model, and the study of defense expenditure policy making is right back where it started.

Since I will contend that the conclusion that Wanat draws from his three empirical criticisms is based on a fundamental misunderstanding of my article, I will briefly review the argument. The article starts with the premise that continually to criticize or defend existing explanations without offering an alternative brings us to an impasse. By offering a model that combines the crucial elements of the existing models and developing a set of evaluative criteria that are policy-relevant, I intend to foster an atmosphere in which future modifications can take place in a constructive and progressive manner.

There is a general consensus that the existing literature is woefully inadequate. In addition to a number of empirical problems (see Ostrom, 1977), the existing explanations ignore the richness of the policy-making process. To overcome these problems, I seek to integrate the existing explanations into a substantively plausible, albeit simple, explanation of defense expenditure policy making using the substantive literature of the years following the formation of the Department of Defense. The Reactive Linkage Model captures the essence of both the Arms Race and Organizational Politics Models, and by doing so increases its explanatory potential.

Once a model is formulated with more explanatory potential than previous ones, the empirical testing phase is designed to determine whether any of the explanatory potential is realized. The goal is not to offer a model that will be immediately useful to policy makers but to determine whether the Reactive Linkage Model is better than existing explanations in terms of criteria of interest to policy makers. The principal conclusions of the empirical analyses are: (1) the substantive hypotheses represented by the individual coefficients are supported; (2) the individual equations provide an excellent fit to each of the four decisions; and (3) the Reactive Linkage Model represents an improvement over existing models.

Wanat's criticisms, which are directed at the empirical analyses and the subsequent interpretation of the goodness-of-fit measures, represent the spirit that led to the previously noted impasse. Consequently, more is at stake here than a mere question of interpreting goodness-of-fit measures, since Wanat's conclusions seek to undermine a model which is offered in the following vein (Ostrom, 1978, pp. 955–56, emphasis added):

> The Reactive Linkage Model, as presently constituted, is by design a compromise between the existing explanations which is weighted heavily in favor of the simpler and more formal explanations. Further research may therefore be directed at increasing the complexity of the explanation in order to more closely approximate the concerns of the Bureaucratic Politics theorist. In this regard, the Reactive Linkage Model can be modified incrementally in response to specific criticisms and/or observations of students of defense policy making. *The current model is but a first step towards integrating existing explanations of U.S. defense expenditure policy making.*

By viewing the Reactive Linkage Model as an impediment to the credibility of students of defense expenditure policy making, Wanat is fundamentally misrepresenting my intent. Although this issue is of primary concern to me, let me first address his three empirical criticisms.

Wanat's first contention is that the Reactive Linkage Model is not very accurate. While I would agree that an RMSE of $4 billion is actually quite large, I do not think that "largeness" can be interpreted in terms of specific programs or weapons systems; my analyses were done with aggregate expenditure data and hence are not relevant to any subcomponent of the defense budget. If the model had been applied to individual components of the defense budget, the absolute magnitude of the forecast error would be substantially smaller. Also, nowhere in the article do I claim that the model is absolutely accurate; all that I claim is that it is relatively more accurate than its competitors. The fact that the absolute magnitude of the forecasting errors is quite large is simply evidence that the Reactive Linkage Model "is but a first step. . ." (Ostrom, 1978, p. 956).

Wanat's second contention is that the naive model test results have been misinterpreted. After looking at the rank order of the forecasting errors, Wanat concludes that the two model's forecasts are indistinguishable. If Wanat is surprised by this outcome, it is only because he does not understand my empirical work (Ostrom, 1977, 1978). Nowhere in my work do I interpret the results of the naive model test as a clear-cut victory for the Reactive Linkage Model. What I do stress is that the Reactive Linkage Model, unlike its competitors, is at least as good as a simple naive model. My conclusion is that the Reactive Linkage Model is better than existing explanations, not that it is of immediate policy relevance.

Even though Wanat misunderstands my intent, his way of looking at the individual forecasting errors underscores an important way in which the Reactive Linkage Model is superior to the no-change naive model. While the Reactive Linkage Model performs better in only 8 of 19 years, it is more accurate in 6 or the 7 years from 1962–1968. One possible explanation is that during periods of rapid growth in the defense expenditure total, the model is more accurate than a naive model; the Reactive Linkage Model incorporates a number of factors into the four policy-making rules and is thereby better able to anticipate defense expenditure levels during periods of rapid growth. If this result can be extrapolated into the future, the Reactive Linkage Model might well provide better forecasts of the 1974–1981 period in which the defense expenditure total has grown very rapidly. Since it is probably the case that social science research has its biggest contribution to make in helping policy makers understand situations in which there is rapid change, I sincerely doubt that "most analysts . . . [would] accept as superior the naive model's predictions."

Wanat's third contention is couched in terms of the following question: "How does the analyst account for the disparity between the poor fit of the Reactive Linkage Model in substantive terms and the 'sharp and plausible' estimates of the parameters in the regression equation?" Underlying this question is a very facile interpretation of policy relevance. To be relevant, a model must tell policy makers something about the environment within which they must operate.

Given the complexity of the U.S. defense expenditure policy-making process, it is not likely that any analyst would arrive at *the* explanation on the first try. Instead, a number of scholars making incremental improvements over previous models will eventually arrive at a relevant model. For this reason, the "sharp and plausible" parameter estimates are very relevant. Any time correlation-based techniques are used to analyze data and estimate parameters, there is a danger of spurious relationships. If the relationships are spurious, one is explaining in a statistical sense without explaining in a substantive sense. Despite Wanat's contention that "academic standards" only require superficial statistical explanation, I am concerned with substantive explanation as well. The fact that the parameter estimates have very small standard errors (i.e., "sharp") and they are consistent with the hypotheses displayed in Table 3 (i.e., "plausible") means only that the forecasts that are generated have more credibility. It does not mean that the forecasts will be absolutely accurate. However, if the model evidences a "poor fit" with "sharp and plausible" parameter estimates, it may mean that incremental modifications to the structural form can improve the forecasting performance.

In addition to the specific problems associated with the three empirical criticisms, Wanat's communication is an example of a type of criticism which impedes scientific progress. According to Lakatos (1970, p. 118), scientific progress consists of a series of progressive problem shifts in the context of an ongoing research program and has both theoretical and empirical components. From a theoretical perspective, a model must both account for the success of previous explanations and increase the explanatory power beyond that achieveable at present. From an empirical perspective, some of the increased explanatory power must be supported.

This perspective implies that criticism, too, must strive for both theoretical and empirical progress if knowledge is to expand. Wanat's critique, when viewed from Lakatos' perspective, is "pseudoscientific" (i.e., destructive rather than constructive). Even if Wanat's conclusion is true, the communication serves no constructive purpose since he makes no effort to improve, in a "progressive" fashion, upon the Reactive Linkage Model. And given the fact that his contentions are largely incorrect, Wanat's communication not only fails to clarify the relationship between academic standards and real-world interpretability but is representative of the type of "hit-and-run" scholarship that only impedes progress toward understanding U.S. defense expenditure policy making.

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Comment on Tufte (Vol. 69, September 1975, pp. 821–26)

TO THE EDITOR:

Edward Tufte (1975) explained the electoral fortunes of the president's party in midterm elections for the house, by

$$V = -11.083 + .133P + .035E + M, \qquad (1)$$

where V is the percentage of the total vote received by the president's party in the general election; P is the president's approval rating as measured with the standardized question asked in the standard manner, before the election, by the Gallup Poll; E is the change from third quarter to third quarter in real disposable income per capita (measured in 1958 dollars) in the year preceding the election; and M is the mean percentage of the total vote received by the president's party in the eight preceding house elections. Tufte (1978) recalibrated the constants and deleted reference to 1958 dollars, with

$$V = -10.74 + .132P + .622E^* + M, \qquad (2)$$

where E^* is the percentage change from third quarter to third quarter in real disposable income per capita in the year preceding the election.

Equation (2) deserves to be called Tufte's Law. This note compares the law with the results of the most recent midterm election and, then, derives some of its theoretical consequences.

The test equation for the 1978 election,

$$V = -10.74 + .132(48.3) + .622\,(2.911) + 53.7 = 51.2, \qquad (3)$$

differs slightly from a preliminary version by Tufte (1978). Democratic party candidates for the house apparently polled 53.4 percent of the total vote in 1978 (*Congressional Quarterly,* 1979). The discrepancy is 2.2 percent; this is somewhat larger than any (previous) discrepancy reported by Tufte (1975). The magnitude of the deviation may be a function of inaccurate data: (1) $P = 48.3$ is the mean of all in-house (not telephone) surveys for September-October, 1978 (*Gallup Opinion Index,* 1978). (We omitted the [presumably telephone] survey of September 19, 1978.) Tufte, by contrast, sometimes averaged and sometimes used only one poll. (2) $E^* = 2.911$ is calculated from figures in *Business Conditions Digest* (1978). Economic indicators, however, are often subject to revision. (3) $M = 53.7$ is the mean of the pertinent figures in *Statistical Abstract* (1974, 1978). Tufte (1978) reported a value of 54.4. His sources are obscure to us, but if his value is accepted, then the discrepancy is reduced from 2.2 percent to 1.5 percent. (The 54.4 was reported before the election, so its honesty [albeit not its accuracy] is self-evident.) We conclude that the fit between model and reality is not so good as ($r^2 = .912$) suggested by Tufte (1975). Nevertheless, given the known shortcomings of available data, the putative results of the most recent midterm election are compatible with Equation (2). Tufte's Law is confirmed.

E and E^* are defined as functions of population, gross national product, and so forth (McConnell, 1972). These econometric definitions enormously expand the analytical power of Tufte's Law. The electoral impact of an

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We are indebted for their criticisms, to three anonymous referees as well as to Professors Harvey Arnold and Roger Marz.

underlying economic variable can be assessed using difference equations. The general technique is illustrated with taxation: The electoral impact of the economic variable E^* is

$$.622E^* = (.622)(100)[(d_2D_2/p_2 - d_1D_1/p_1)/(d_1D_1/p_1)], \qquad (4)$$

where d_i is the deflator, D_i is the disposable income, and p_i is the population in successive third quarters. Disposable income, by definition, equals personal income minus personal taxes. Thus, by substitution,

$$.622E^* = (.622)(100)[(d_2(I_2-T_2)/p_2 - d_1(I_1-T_1)/p_1)/(d_1(I_1-T_1)/p_1)], \qquad (5)$$

where I_i is personal income and T_i is personal taxes at successive third quarters. For an assessment of the consequences of the level of personal taxation, $d_1 = d_2$, $I_1 = I_2$, and $p_1 = p_2$. (This *ceteris paribus* is analytic; partial derivatives are the calculus counterpart.) Equation (5) thus reduces to

$$.622E^* = (.622)(100)(T_1 - T_2)/(I - T_1), \qquad (6)$$

where $I = I_1 = I_2$. Equation (6) has a qualitative corollary: An increase in the level of personal taxation ($T_2 > T_1$) decreases the percentage of the total vote received by the president's party. The electoral effect of any change in the level of personal taxation can be computed in advance, since the existing levels of personal taxation (T_1) and personal income (I) are known a year before the election. With a little algebra and econometric definitions, this difference equation technique applies to other economic concepts. The resulting theorems are not all self-evident.

Tufte's Law is a good first approximation and, when supplemented, a powerful theoretical mechanism. It should not be disparaged because it might be limited to the United States House of Representatives. Johannes Kepler, after all, described only one planetary system.

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Comment on Frohlich, Oppenheimer, Smith and Young (Vol. 72, March 1978, pp. 178–97)

TO THE EDITOR:

A number of years ago, Samuel Popkin (1974) wrote a somewhat less than enthusiastic review of my book *Computer Simulations of Voting Behavior* (1972). More recently, Norman Frohlich, Joe A. Oppenheimer, Jeffrey Smith and Oran R. Young (1978) echoed Popkin's concerns, but reported findings which lend credence to a major premise of my models that voters display rationality in their decision making. I would like to respond to a couple of their major criticisms, with special reference to the Frohlich et al. article.

One of their chief concerns lies with my operationalization of the party differential of Anthony Downs' model of voter decision making (Downs, 1957). In my simulation, the basic indicator of the party differential was the respondent's reported perception of which party would enhance his/her personal financial situation. Since this is a narrow definition of an individual's self-interest, I am sympathetic with Frohlich's desire to develop a broader measure of the party differential, in order to test another operationalized version of the Downsian model. However, the resulting index may be so inclusive as to incorporate factors which are perceived to do little in the way of enhancing an individual's utility income.

The party differential index used by Frohlich and his colleagues involves the addition of three factors: (1) which party is expected to do the best job of handling the most important problems confronting the government, (2) the perception that one party may do a little better job enhancing the respondent's financial well-being (the same measure employed in my study), and (3) the individual's party identification weighted by his/her exposure to the media.

The first component is tapped by a number of survey items beginning with the 1964 SRC variable #36 which reads as follows:

> As you well know, there are many serious problems in this country and in other parts of the world. The question is, what should be done about them and who should do it. We want to ask you about the problems you think the government in Washington should do something about and any problems it should stay out of. First, what would you personally feel are *the most important problems the government should try to take care of* when the new President and Congress take office in January?

Then the respondent is asked about his/her degree of concern and which party will more likely take the course of action he/she prefers. The SRC also posed questions which explored attitudes regarding the second and third most important problems.

While basing a vote on an assessment of which party will best deal with the most important problem confronting the government is certainly rational and responsible in the general sense, it appears to me that the self-interest axiom embedded in Downsian logic may be sacrificed. Although a citizen may believe that the government should deal with the most troublesome matter, there is no explicit statement that his/her utility will be altered materially by a change in party control of the presidency and Congress. Indeed, utility may be enhanced, but there is no indication that the individual perceives that to be the case. Such a perception is central to Downsian theory.

The second component of the Frohlich et al. party differential measure regarding financial well-being has been discussed sufficiently. Clearly, the respondent can indicate whether he/she will benefit from a given party's control of the government.

The third component, party identification, is also an integral part of the party differential index computed by Frohlich et al. Now, "usually thinking of yourself" as a Democrat or a Republican hardly stands as a definitive statement about utility flows which may be affected by an impending election.

However, Downsian logic allows for "habitual" party voting, assuming, of course, that the individual had gone through a careful assessment in a previous election period and had decided not to become "well-informed, unless some catastrophe makes them realize it no longer expresses their best interests" (Downs, 1957, p. 85).

Since Downs' theorizing ignores the political socialization process through which so many learn party identifications in advance of their eligibility to vote, one could raise a legitimate question about the empirical validity of his explanation of the development of "habitual" voting. However, even if we assume that all *individual* partisan attachments are a product of the rational thought process described by Downs, we must also assume, with Downs, that they vote out of habit "without becoming well-informed." Yet, under the Frohlich approach, a person can read newspaper articles about the campaign *often*, listen to *several* radio programs about the campaign, read *several* magazine articles about the campaign, and watch *several* TV programs about the campaign and still have a portion of his/her party identification added into their party differential!

In the future, even more serious doubt may be cast on the use of party identification as an initial estimate of party differential. Given declining partisan attachments and a greater incidence of ticket-splitting, the Downsian voter may determine that his/her party's presidential ticket may do less to enhance his/her utility than the opposition's nominee. The first step then requires a determination of whether or not the individual views one party as superior in maximizing his/her benefits. If instead this citizen opts for "habitual" voting as a means of cutting costs, then party loyalty would be a reasonable substitute for the differential.

Of course, it is no criticism of Frohlich et al. to note that ticket-splitting creates a real problem for Downs, since he assumes explicitly (1957, p. 25) that "every team [party] member has exactly the same goals." Significant intraparty policy differences may prompt a voter to support one of a party's candidates and oppose another.

In any event, substituting party identification for the party differential would be quite a different matter from using party loyalty, as measured by the SRC, as a factor in calculating a party differential.

In summary, while Downs indicates that "self-denying charity is often a great source of benefits to oneself" (1957, p. 37), one cannot assume, nevertheless, that upon the discovery of a person engaged in an act of self-denying charity, we have also unearthed a citizen who expects to benefit personally. Such may be the case, but one needs an explicit statement that the individual under study sees it that way.

Another major criticism of my work involves the notion that information costs aren't voting costs in the Downsian world. However, Downs argues (1957, p. 265) that "time is the principal cost of voting: time to register, to discover what parties are running, to deliberate, to go to the polls, and to mark the ballot."

I have assumed that the time to deliberate *is* a major cost, and that potential voters *are* deliberating when they consume information about a campaign. At a minimum, assimilation costs are incurred when incorporating information for purposes of assessment. Thus, one can distill from Downs the notion that information costs represent a principal form of voting costs. Empirically, the use of this type of cost does not receive much support in the findings. Thus it should be dropped.

Perhaps, as Frohlich et al. appear to suggest, the time spent deliberating really isn't a voting cost, and therefore, other more appropriate measures of the cost of voting could be devised for further testing. However, Frohlich et al. decline the opportunity. Instead, preference and turnout are analyzed separately. This may offer a partial explanation for superior prediction levels. Simultaneously predicting choice and turnout may be a more difficult task than forecasting one or the other.

Other strategies also may have increased predictive accuracy. For example, the authors indicate in footnote 27 that all respondents whose party differentials were zero were dropped from the analysis, cutting dramatically the size of the sample. But, alas, Downs argues that a great number of individuals with zero party differentials vote in elections. Those who perceive that the parties offer identical platforms or policies, use a performance rating to make a selection. For those who anticipate equal utility from the parties irrespective of policy differences, and who also expect benefits in the form of the long-run participation value to exceed costs, will vote for a party chosen at random. In this latter case, when choice is reduced to a coin-tossing operation, predictive accuracy will be lost.

In summary, Frohlich and his colleagues attempt to improve upon my operationalizations of the Downsian theory and apply a different methodological strategy. As a result, Downs' overall model is segmented to the point that many important theoretical linkages are left unexplored. For example, individuals with zero party differentials are deleted rather than being subjected to the logic specified by Downs. Consequently, while Frohlich et al. have produced an interesting study, the operationalized model employed does not bear a close resemblance to Downsian theory.

Regrettably, because of the very nature of this sort of communication, a reader may draw the inference that I am completely critical of the study by Frohlich and his colleagues. Such is not the case. I believe their article represents an interesting treatment of rational-actor explanations and contributes to the scholarly discourse on a most important subject. I am particularly sympathetic to their view that even the SRC six-component model is best explained in terms of Downsian theory. V. O. Key (1966, p. 7) was right when he asserted that voters aren't "fools."

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Reply

TO THE EDITOR:

William Shaffer is right in alluding to a strong intellectual tie between Popkin's (1974) review of Shaffer's book (1972) and our work on the Downsian voter (1978). But it would be a shame if our work were to be interpreted as an attack on that of Shaffer. After all, his was the first attempt to examine the overall empirical fit of the Downsian model of voting. It is always easier to criticize and improve than to pioneer. Originally there were a number of rather fundamental differences between our attempt to test Downs and Shaffer; most of these seem no longer to divide us. What is left are a few questions of operationalization. Although such questions should not be minimized, neither can they be resolved without further empirical work designed to test the implications of each of the proposed measures. Of course, not all of Shaffer's criticisms are of this sort. We find most of the others erroneous. Unfortunately, they do not address the more serious issue underlying the research. Consider Shaffer's points.

1. He believes that our index of party differential is inappropriate because the subjec-

tively identified "most important problems facing the government," which are central to the index, are unattached to the self-interest postulate of Downs. As Shaffer puts it: "While basing a vote on an assessment of which party will best deal with the . . . problem is certainly rational and responsible . . . there is no explicit statement that (a citizen's) utility will be altered materially by a change. . . ." It is difficult to understand how Shaffer reaches this conclusion. The issues used in the index are those that the voters themselves "personally feel are the most important." Moreover, we modified the responses to these survey questions by the degree both of "worry" that individuals indicated this issue caused them, and of perception of relevant party differences on this problem. Alleviation or minimization of worry is of positive private benefit to the individual and must be considered as an expected change of future utility.

Precisely how broadly or narrowly one defines self-interest, of course, is still to be resolved. Shaffer's measure of party differential (the degree to which personal finances are at stake) is clearly self-interested, yet very confining. It is surely narrower than Downs' discussion would warrant. "It is possible for a citizen to receive utility from events that are only remotely connected with his own material income. . . . There can be no simple identification of . . . acting for one's own greatest benefit with selfishness in the narrow sense. . ." (Downs, p. 37). If our broader measure is thought to confuse the issue, one needs to examine the evidence. One relevant subset of voters is that which had the narrower (financial) conceptions of self-interest at stake. Did our measure cause the predictions of which party would receive the vote to deteriorate for this subset of voters? No (1978, p. 191). Indeed, for this subset very few changes in predictions occurred. Rather, already fine predictions were slightly improved by using our broader operationalizations for this subgroup of the sample (N = 361). Of course, the major consequence is that with the broader operationalization, one is able to make predictions for three times (N = 1067) as many individuals in the sample.

2. Shaffer claims that our weighting of party identification inappropriately allows for a residual party identification effect to remain even when one has considerable information. But where should one have the threshold? In our model the impact of party identification is increasingly discounted as the voter obtains more information. The residual impact on a "well-informed" voter is minimal—in the instance cited by Shaffer contributing only 1.03 units to an overall possible party differential of 16 units. Is this 1.03 units likely to change our predicted behavior for such an individual? 1.03 can only change the prediction if the individual were either relatively evenly cross-pressured on the issues or if the voter perceived only one issue of concern (not the financial one!) and was no more than "fairly worried" about this issue. Again, it appears to be a question of judgment as to whether in such situations party identification should be permitted to be considered or whether this does violence to Downs' conception of the rational voter. We personally do not believe major issues are at stake here. Indeed, information is the one variable which seems to add very little to the analysis (p. 191). Adjusting the weighting, therefore, would not seem to be very central to the question of improved operationalization.

3. Shaffer argues that party identification should not automatically be used in future applications of Downsian models to American politics. Some analyses of 1972 election results have called the importance of this variable into question. Nevertheless, 1976 results have indicated the re-emergence of a party-related factor is an influence on voting behavior. It is conceivable that party identification is declining among voters while the use of party as an information factor in voting decisions persists. We agree that these observations should be considered by future analysts.

4. Shaffer quarrels with the way in which we deal with the issues related to the cost of voting. Certainly if we had been able to predict combined decisions of turnout and party choice for each voter, our accuracy would have suffered. But given the falsification of the equal cost assumption (pp. 187–88), and the lack of cost measures in the data set, such combined predictions were not justified. The interested reader should examine Jeff Smith's recent arguments (1978). It is a shame that Shaffer reopens the question of deliberation costs as a cost of voting. Of course such a cost exists. But it need not deter one from going to the polls; rather, it only deters one from getting information. Once these costs have been borne, one would expect those who already have invested in information to be more, rather than less, likely to vote. Shaffer's study assumed the opposite: that because they consumed information they had a higher cost of voting and were thus less likely to go to the polls! The cost of the act of voting must be carefully separated from the investment entailed in voting on an informed basis.

5. Shaffer objects that we dramatically cut

the size of the sample by leaving out those voters with a zero party differential. We made predictions on turnout for our sample of 1100. Only 33 of these had zero party differential (3 percent) and were dropped for purposes of predicting the direction of their vote.

It is, of course, unfortunate that the larger issue has not been raised: if one uses this sort of data to test Downs, which aspects of the model remain untested? These data do not go to the heart of the voter's rationality. They do not directly test for the overall choice process of the individual (are the preferences transitive? etc.) which are at the base of the theory (Frohlich and Oppenheimer, 1978, pp. 1–14). Nor does it definitively answer questions of self-interest. There is growing evidence that the rationality assumption cannot be sustained in a variety of choice situations. (For an overview of recent work see Grether and Plott, 1979.) Thus the predictive promise of the Downsian approach must be contrasted with the strict falsity of the theory. Perhaps the most fundamental issue is whether it is epistemologically justifiable to pursue alternative operationalizations in the face of potentially false assumptions. Some (such as Miller, 1975) would say no. In the absence of definitive results in this area of application, however, the issue is still open and should be addressed in future work.

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Comment on Spegele
(Vol. 74, March 1980, pp. 104–22)

TO THE EDITOR:

I am in close agreement with Roger D. Spegele's recent essay, except for his interpretation of what I have written. Spegele claims that I reject induction in favor of deduction and testing (p. 104). In the sentence before the one he misconstrues, however, I wrote as follows: "The point is not to reject induction but to ask what induction can and cannot accomplish." In this and other matters, Spegele wrongly identifies my views with those of a school I have not joined. I appreciate having the chance to make this clear.

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EDITORIAL NOTE

The availability of data from the 1978 congressional election study has resulted in the writing of many important papers. Several were presented at the Association meeting in September 1979, several more in January 1980 at a conference in Houston ably directed by David W. Brady (University of Houston) and Joseph Cooper (Rice University). In this issue we publish two of the papers in this first wave. In addition, Barbara Hinckley was asked to prepare a refereed comment analyzing the progress and prospects of this research.

The following articles have tentatively been scheduled to appear in the December, 1980, issue:

Bruce Bueno de Mesquita, University of Rochester, "An Expected Utility Theory of International Conflict: An Exploratory Study"

Valerie Jane Bunce, Northwestern University, "The Succession Connection: Policy Cycles and Political Change in the Soviet Union and Eastern Europe"

Charles F. Doran, Johns Hopkins University, and Wes Parsons, Rice University, "War and the Cycle of Relative Power"

Arthur M. Melzer, Michigan State University, "Rousseau and the Problem of Bourgeois Society"

Kenneth R. Mladenka, Texas A&M University, "The Urban Bureaucracy and the Chicago Political Machine: Who Gets What and the Limits of Political Control"

David R. Morgan and John P. Pelissero, University of Oklahoma, "Urban Policy: Does Political Structure Matter?"

Benjamin A. Most, Brown University, and Harvey Starr, Indiana University, "Diffusion, Reinforcement, Geopolitics, and the Spread of War"

Yoav Peled, University of California, Los Angeles, "Rousseau's Inhibited Radicalism: An Analysis of His Political Thought in Light of His Economic Ideas"

Clarence N. Stone, University of Maryland, "Systemic Power in Community Decision Making: A Restatement of Stratification Theory"

Stephen K. White, University of Hartford, "Reason and Authority in Habermas: A Critique of the Critics"

BOOK REVIEWS

Political Theory and Methodology

Political Theory and International Relations. By Charles R. Beitz. (Princeton, N.J.: Princeton University Press, 1979. Pp. ix + 212. $16.50, cloth; $3.95, paper.)

"Political theory" is a notoriously ambiguous expression. It has been used to refer to the attempt to think systematically about politics, to explain political phenomena, to defend moral judgments of political conduct, and to analyze political discourse. This ambiguity is only slightly diminished by attaching the qualifier "normative" to "political theory," for it is still unclear whether to engage in normative theorizing is to examine the presuppositions of "normative" discourse or to participate in that discourse. Indeed, one might argue that to speak of "normative political theory" is to confuse the making of moral judgments with the analysis of moral judgment–i.e., to confuse the practical and the theoretical. According to this view, "normative theory" is a contradiction in terms, and the appropriate answer to the question with which Charles Beitz begins his book–"Is normative international political theory possible?"–is a short "No." We need not even begin to worry about the "international."

As Beitz has gone on to write an interesting book about international morality, one must conclude that he has simply mischaracterized his endeavor. What Beitz really means by "political theory" is "moral judgment." The first part of his study defends the possibility of moral judgments concerning international affairs against a variety of skeptical arguments. Beitz's main target is the view that morality has no international application because the relations of states take place in a "state of nature" in which no state can have an interest in conducting itself according to common rules. This condition is one in which each state is autonomous, and the most inclusive morality is therefore that of reason of state. Beitz questions the empirical premise of this conception, arguing that there has always been some degree of common interest among states and that the growth of global interdependence and transnational interactions means that the postulated autonomy of the state is becoming increasingly divorced from the actual condition of international society. Beitz also attacks the common attitude that international moral principles must be justified by showing how adherence to them promotes the interests of states, arguing–correctly, I think–that to adopt such a view of international morality is to commit oneself to skepticism concerning all moral judgment. International morality is simply moral reasoning that takes account of the circumstances of international affairs.

The conception of moral judgment upon which Beitz relies is based on the ideas of John Rawls. From a revised Rawlsian perspective on social justice Beitz attacks the main alternative to international moral skepticism, a "morality of states" derived from a conception of the world as an association of independent and self-sufficient political communities. The morality of states is based on the principle of state autonomy and on its correlative, the principle of nonintervention. But, argues Beitz, states are not in fact self-sufficient, and there can be no right to autonomy except as it serves to promote both domestic and international social justice. The main limitation of this argument is that it presupposes a conception of social justice that is not itself defended in the book. Beitz goes on to criticize as arbitrary Rawls' own application (in *A Theory of Justice*) of the principles of justice to states, and to defend the view that these principles must be applied globally to individuals. Rawls' account of international social justice is inadequate because it depends upon the same assumption of self-sufficiency that vitiates both international skepticism and the morality of states. The correct view of international morality, Beitz suggests, is a cosmopolitan one in which the principles of social justice apply to mankind as a whole, and only derivatively to states. The reader will have to judge whether Beitz succeeds in making the case for this particular version of cosmopolitanism.

The book ends with the observation that "such systematic moral debate about international relations as has taken place has been between adherents of international skepticism and the morality of states. However . . . the more pressing issues are those that divide the morality of states from a cosmopolitan morality" (p. 183). This is exactly right. One need not accept Beitz's arguments for a cosmopolitan morality rooted in the idea of social justice to agree that he has succeeded in clarifying the agenda of moral debate concerning the relations of states and of their inhabitants.

TERRY NARDIN

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The Biology of Peace and War: Men, Animals, and Aggression (Krieg und Frieden aus der Sicht der Verhaltensforschung). By Irenäus Eibl-Eibesfeldt. Translated by Eric Mosbacher. (New York: Viking Press, 1979. Pp. viii + 294. $15.00.)

All biological analysis of political behavior, including the violent kind, is concerned with the innate, organic portion of the basic causes of overt human acts. That is, such analysis deals ultimately with the genes, which constitute what is called human nature.

Biological studies, as they have developed in recent decades, have adopted three modes of analysis. One is ethological, in which the behavior of usually intact vertebrates (from fish to fowl to primates, including human beings) is observed in usually natural, uncontrived circumstances. Another is sociobiological, in which animals with primitive neural development (mainly insects) are studied in both natural and experimental circumstances. A third is physiological, in which the focus of investigation is the vertebrate brain, to learn how its two major control systems—the central nervous system and the endocrine system—affect overt behavior.

Ethologists thus make their inferences about genetic forces by observing overt behavior many giant steps from the genes. Sociobiologists make theirs close to the genes but many giant steps from human behavior. Physiologists make their inferences much closer to actual causes, observing the effect on overt behavior of surgical, electrical, or chemical intervention. When you put an electrode into the brain of a chicken or a cat and the animal makes an attack immediately after you apply current to the electrode, you can reasonably infer that the current caused the attack. When you insert a tiny tube into a monkey's brain and then inject a compound that destroys adrenaline, you can reasonably infer the part that adrenaline plays in conflictful interaction.

Eibl-Eibesfeldt provides the best, most wide-ranging array of evidence that I have seen in ethological writing, but no matter how much evidence is gathered, the inferential distance remains great. When you observe dominant male monkeys take their fill of food first, before letting females and subordinate males get any, does this demonstrate or even more than suggest that all male vertebrates have an innate desire to dominate and that it overrides any other innate predispositions? Or does it indicate that the observer has been looking at overt dominance rather than its organic causes?

There is a similarly large gap between research and inference in sociobiology. When queen ants and bees in some species kill and praying mantis females eat their sex partners after their once-in-a-lifetime act of love, does this demonstrate that females are selfish and androphagous or that they are altruistic because they are recycling protein and preventing the deterioration of the species that would occur if their mates lived to breed another day with an inferior male?

Konrad Lorenz, the mentor of not only Eibl-Eibesfeldt but also other writers (like Robert Ardrey) and distinguished scientists (like Eckhard Hess) had a pessimistic view. He thought that human beings are rather uniquely aggressive predators, killing their conspecifics like no other species, in sustained and savage genocidal wars. Eibl-Eibesfeldt takes an optimistic view. While accepting his mentor's assertion that aggression is an innate drive, he emphasizes what can be done to control it.

Both pessimists and optimists, among not only ethologists and sociobiologists but also their adherents, do not consider whether they themselves may have been predetermined to be pessimistic or optimistic, perhaps as a reaction to living in the twentieth century, with its enormous burden of internal and international conflict. And so everyone feels free to ignore such nonrational forces that affect their observations and conclusions. People now remain as "free" as Hobbes was in the seventeenth century to conclude that seeking dominance by aggressive acts is inherent in human nature, or as Rousseau was to conclude pretty much the opposite a century later.

But there is a less psychological explanation for the ease with which ethologists and their adherents have produced such inconclusive conclusions. With rare exceptions, like J. P. Scott and Eckhard Hess, ethologists have been so fuzzy about their definitions that it has been easy for them to plead misinterpretation. Aggression, chronically ill-defined, according to Eibl-Eibesfeldt, following Lorenz and Scott, means agonistic behavior, which also is ill-defined. Or it means fighting, a definition about which there can be readier consensus. However, like Lorenz but unlike Scott, Eibl-Eibesfeldt remains fuzzy about levels of analysis, and he rejects intent as an ingredient of aggression. This makes it rather difficult to distinguish the act of love from the act of rape, the scalpel of a heart surgeon from the well-aimed knife of a murderer. May we not more exactly define aggression as an action-tendency which is intentionally harmful?

The means-end relationship between aggression and basic, innate drives remains altogether unclear in this book. The author at some points regards the relationship as functional (as when an animal defends territory or seeks a sex partner), but elsewhere it is indefinitely related to what he calls "innate release mechanisms" (p. 10), including among these sex and fear of strangers (pp. 48–49) and basic needs like food, sleep, and sex (p. 120). And he repeatedly refers to what he regards as the innate desire of individuals to establish rank-order. However, he argues that rank-order is not a goal but may provide a socially useful means of controlling aggression (pp. 46–48, 84–87).

Eibl-Eibesfeldt notes that the Chinese revolution tried to establish equality by force. Since this runs counter to the (innate?) desire to be both superordinate and subordinate, we should let rank-order be established and seek only to limit its harmful effects (p. 235). He adds that the Judeo-Christian equality principle "has made a vital contribution to the pacification of the world" (p. 235). But he does not consider whether the denial of human equality and dignity, as Jefferson and some latter-day revolutionists have asserted, is a just cause for conflict. That is, he ignores the question whether equality is an innate need or innate release mechanism, while arguing that rank-order is somehow ultimately innate.

Are there then any innate inhibitors to aggression and its derivative, rank-order? Eibl-Eibesfeldt says sympathy is one, thereby leaving uncertain the question whether sympathy, not usually considered a mechanism that releases innate aggression, itself releases some other innate drive or *is* an innate drive. Not only are the dynamics of innate drives and release mechanisms unclear, but another set of innate forces appears: norms. Some of these are innate and universal and others "culturally transmitted" (pp. 193–94). It is unclear whether norms are derivations of innate release mechanisms or of sympathy or are a different kind of genetic tendency.

The research and writings of ethologists and sociobiologists do not seem to me to provide much more than a scientific patina for the kinds of fundamental assumptions about human nature that have been made—usually with far greater intellectual rigor—by Hobbesian pessimists and Rousseauean optimists. The work of physiologists, on the other hand, seems to me to be far more promising and from it we can get a much clearer picture of genetic forces. If you can discover that testosterone and adrenaline are related to assertive action (which may have constructive or destructive intent) and its subset, aggressive action (which has destructive intent) and if you can ascertain the amounts of these hormones that are critical to producing such action, then you are closer to ascertaining the innate causes of conflict. This is recognized by the most impressive of the sociobiologists, E. O. Wilson, who says on the last page of his massive *Sociobiology* (1975, p. 575) that "the transition from purely phenomenological to fundamental theory in sociology must await a full, neuronal explanation of the human brain."

If, as seems evident and as Wilson says, we must await a full neuronal (and hormonal) explanation, then those who are impatient and intolerant of ambiguity will continue to find persuasive evidence for whatever view of human nature they started with. Ethologists and sociobiologists, like social scientists, lack the patience and the training to undertake or even to understand what physiologists are up to. And physiologists themselves, following in the shadow of such great pioneers as Walter Cannon, mostly assume a view of human nature which, like their view of vertebrate nature, limits the basic drives to self and species preservation. It is rather evident that it will take a new generation of biologists and social scientists to address themselves fruitfully to the basic questions that ethologists so seriously but with much confusion address themselves to.

JAMES CHOWNING DAVIES

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Through the Looking-glass: Epistemology and the Conduct of Inquiry—An Anthology. Edited by Maria J. Falco. (Washington, D.C.: University Press of America, 1979. Pp. viii + 409. $13.75, paper.)

At the 1975 APSA Annual Meeting, seven panels were organized by Maria J. Falco for a section entitled "Epistemology and Methodology." Papers and discussion from five of these panels are brought together in this anthology.

Since convention papers often stray from their assigned topics and seldom are presented in publishable form, the reader may well approach a collection of this sort with low expectations. In this case, the papers are on the whole good ones, the discussion is pertinent, and the original materials have been edited carefully by Falco, who has also written a helpful introduction. While the papers deal with issues of cognition in political inquiry, they do not address the central problems of epistemology as directly and fully as one might wish. The anthology is thus of greater value for what it tells us about the concerns and commitments of a representative group of political theorists in the mid-1970s than for its contribution to epistemology per se.

The volume opens with a round-table discussion, chaired by Eugene Meehan, on "The Nature of Political Inquiry." Participants in the discussion are Stephen Toulmin, Warren Sussman, Gwynn Nettler, Fred Eidlin and David Edwards. The second panel, "Epistemology and Political Ideology," consists of papers by Alasdair MacIntyre and David Braybrooke and discussion by Fred Dallmayr. The third panel on "Alternative Methodologies for Political Evaluation" includes papers by John Gunnell, Fred Frohock, Dante Germino and Paul Kress, with discussion by Felix Oppenheim. The fourth panel, "Rational Versus Cultural Bases of Political Inquiry," features papers by William Bluhm, William Irvine, Dennis Palumbo and Stanley Renshon, with David Schwartz as the discussant. In the final panel, "Ways of Seeing and Ways of Knowing Democracy," Joel Kassiola, Elliott White, and Samuel Hines, Jr., present papers and Maria Falco serves as the discussant.

It is not possible here to consider each of these presentations individually, but some impressions can be given of the collection as a whole. First, this anthology contains very little discussion (except for the papers of Braybrooke and Hines) of what epistemology is and what epistemological speculations aim to accomplish. We are thus left to wonder if the various authors are talking about the same thing when they speak of "epistemology" and if they accept the distinctively modern view that epistemology must take priority over the study of being and provide a foundation for the special sciences.

Second, the precise relationship of political science to epistemology is left in some obscurity. The case can be made (as Kress has shown in a subsequent article in the May, 1979 *Journal of Politics*) that the preoccupation with epistemology has been harmful to political science. Why is political inquiry compelled to reflect on the character of human knowledge? Is it dependent on epistemological speculation? How is it benefited or hindered by epistemology? The anthology suggests various answers to these questions, but it does not take them up thematically.

Third, a strong current of opposition to logical positivism or logical empiricism runs throughout the anthology. Even Oppenheim grants (in commenting on Gunnell's paper) that "the philosophy of logical positivism has been thoroughly discredited" (p. 177). Several contributors maintain that facts are determined by the observer's conceptual framework, and Oppenheim concedes the point: "We cannot capture facts except through our conceptual lenses" (p. 177). One wonders, however, if the implications of such an assertion have been fully recognized. In rejecting Frohock's defense of evaluative reasoning, Oppenheim reaffirms his own "value noncognitivism," even though his formulation of this position in earlier writings had seemed to require a positivist account of facts.

Fourth, the status of "concepts" emerges as the crucial issue, once facts are made to depend on concepts. It seems that a majority of the contributors might prefer the view that political reality is constituted by concepts to the view that there is an independent political reality which concepts reflect. Eidlin, in his contribution to the round-table discussion, and Dallmayr, in his comments on MacIntyre's paper, warn that the constitutivist viewpoint may lead to an epistemological relativism, in which no place remains for truth as a standard of discussion. Mindful of this danger, Braybrook insists that his "new epistemology," which builds on post-Wittgensteinian insights into language, leaves room for "objectification" in social science, and Hines maintains that his "evolutionary epistemology" can avoid a Kuhnian relativism. From Germino's fine exposition of Voegelin, we learn that this philosopher reaffirms the ancient view that reality is the measure of man's symbols of order. Nevertheless, Voegelin regards reality itself as a "process" or "moving presence." The structure that reality exhibits, rather than being something fixed or permanent, is "moving beyond itself" (pp. 133, 141). Philosophers must seek to understand this process from within, and they cannot hope to discern its meaning as a whole. Thus "the answers to the basic questions of human existence remain shrouded in mystery" (p. 134). Perhaps Voegelin's position is not so different as one first supposes from "evolutionary epistemology," which—in Falco's words—views reality as "a moving target" (p. 371).

Finally, the discussion remains for the most part within the framework of twentieth-century epistemological debate, and not even the leading contemporary philosophers—Husserl, Wittgenstein, Heidegger—are examined seriously. No attention at all is given to the great political philosophers, who were also the great epistemologists and whose writings exhibit with special clarity the political relevance of inquiry about knowing.

EUGENE F. MILLER

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Political, Electoral, and Spatial Systems: An Essay in Political Geography. By R. J. Johnston. (New York: Oxford University Press, 1979. Pp. viii + 221. $19.95, cloth; $11.95, paper.)

R. J. Johnson's book is the compelling reason why it is time political scientists paid attention to geography. The new geography does not recognize the discipline as essentially descriptive; now geography is the study of the relationship between human behavior and space. "Spatial models" are not built of the mathematical spaces of our model builders; space is "real space," the three-dimensional kind in which one of us is here and another there. So broad an organizing concept for a discipline leads one to expect to find it populated by synthesizers as well as researchers. And Johnson, a British-trained New Zealander, whose research is highly respected among geographers, proves himself an excellent synthesizer in his latest effort. While the book carries a measure of unnecessary ideological baggage, it largely succeeds in bringing together Easton, Downs and a host of empirical work on local government and electoral systems.

The book presents and supports a theory of "the geography of well-being," which is a theory of who gets what, when, how, but most of all, where. Johnson presents no new measure of well-being and he does not deal with the literature of social indicators. The theory does not identify a singly measured dependent variable, but does consistently attempt to explain well-being. The theory focuses on Great Britain and the United States.

The theory holds that the overwhelmingly important independent variable accounting for well-being is the "spatial division of labor." Where workers live, how the resources of the society are distributed among workers, and what options are available to them is at once caused by the capitalist system and causes most of the political conflicts in a society. Johnson attributes the spatial division of labor to capitalists' "never-satisfied quest for power" and he makes little of the effects of a market economy.

Other main determinants of the geography of well-being are the physical environment—all the various resources available to a society—and the proximate environment—the actual spatial arrangement of human beings in the society.

Finally, but not least emphasized, is the role of government. Here Johnson emphasizes the use of government by the capitalist class to protect its own interests. In a concluding chapter he argues, in agreement with middle-class reformers who think that a bureaucrat who sees what is best for society will do it, for the "removal of flexibility in the operation of many government programmes so that politicians, particularly those in local governments, become much more like administrators operating within closely prescribed rules."

Johnson is not a determinist, but he is not a wild-eyed reformer either. His dynamic theory of well-being has as its antecedent independent variable, political action. Political action affects well-being in two ways: it makes direct demands on the system; and it makes indirect demands through the electoral system which translates and frames themes. In the context of this model he suggests a number of familiar reforms.

Neither the familiarity of the reforms and the synthesized concepts, nor the unfamiliarity of the discipline should deter serious students of elections or local government from reading this fine book.

JOHN C. BLYDENBURGH

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Main Currents of Marxism: Its Rise, Growth, and Dissolution: Vol. 1, **The Founders;** Vol. 2, **The Golden Age;** Vol. 3, **The Breakdown.** By Leszek Kolakowsky. Translated by P. S. Falla. (New York: Oxford University Press, 1978. Pp. xiii + 434; viii + 542; xii + 548. $19.95, each.)

In a recent discussion of historical materialism, Jürgen Habermas distinguishes among restorationist, renaissance, and reconstructive approaches to Marx's theories. These approaches could be exemplified by Lenin's determination, in *State and Revolution,* to restore the authentic political teachings of Marx and Engels after their corruption by later Marxists; the renewal of interest in Marxist thought after its cold-war proscription in the United States; and Habermas' own attempt to "reconstruct" Marx's theory of social evolution from the foundations up. Because Habermas views Marx as an heir of the Enlightenment, he does not regard reconstruction as alien to the latter's outlook. Yet a fourth orientation might be added to this list:

deconstruction. To the history of deconstructive investigations of Marxism Kolakowski's three-volume study makes a notable contribution.

The author is well equipped to undertake this task. Formerly a leading Marxist dissident at the University of Warsaw, he now criticizes Marx and Marxism from a very different vantage point, at Oxford University. Having tried his hand at revisionism, Kolakowski would now like to put a close to what Karl Korsch once called the "Marx-Lenin-Stalin episode." To what extent a single-authored, selective, intellectual history of the "episode" can contribute to its termination cannot be adequately assessed in the scope of a short review. Instead, following a description of these volumes' contents, I will offer only general critical remarks.

As its subtitle, *The Founders,* indicates, Kolakowski's first volume concentrates on the thought of Marx and Engels, But many related thinkers are treated here. After a long opening chapter on "The Origins of the Dialectic," from Plotinus to Hegel, the author works his way through the Young Hegelians, the very early Marx who shared their milieu, and the German Communist Moses Hess, to the main works of Marx's youth. Chapter 8, on *The German Ideology,* includes a useful section on Max Stirner, to whom much of this lengthy work was devoted.

Having reconstructed the "philosophical" evolution of Marxism down to 1846, Kolakowski retraces his steps to take in the prior history of (for the most part French) socialist thought, from Babeuf to Proudhon. This section concludes with a discussion of *The Communist Manifesto.* The author then turns to the evolution of Marxism after 1847. He provides sketches of Marx's antagonists, Ferdinand Lassalle and Mikhail Bakunin, as well as remarks on Marx's political activities. The rest of *The Founders* examines the following topics: Marx's critique of the political economy of capitalism (Chs. 12–13); the more general theory of historical materialism (Ch. 4); and the later Engels' broad reflections on nature and the dialectic (Ch. 15). The volume concludes with a critical overview of Marx and Engels.

The Golden Age of Marxism is identified with the era of the Second International (1889–1914). This volume, like much of the next, proceeds by taking up individual writers in turn. Kolakowski opens with three chapters on the leading protagonists of Second International Marxism, Karl Kautsky, Rosa Luxemburg, and Eduard Bernstein. Of Luxemburg it is said that she "is an outstanding example of a type of mind that is often met with in the history of Marxism and appears to be specially attracted by the Marxist outlook. It is characterized by slavish submission to authority, together with a belief that in that submission the values of scientific thought can be preserved" (p. 94). Evidently, Bernstein did not share this defect (p. 105). He turns next to some writers who have often been played down in "orthodox" histories, or whose connection to Marx has been regarded as tenuous: Jean Jaurès, Georges Sorel, Paul Lafargue, Antonio Labriola, and the Austro-Marxists (Otto Bauer, Max Adler, Rudolf Hilferding). Three lesser-known Polish Marxists are introduced in separate chapters; their inclusion throws light on a subhistory of Marxism that is not well known in the United States. The concluding two-fifths of the volume is dedicated to the origins of the (for our century) most important offshoot of Second International Marxism: Bolshevism. Kolakowski examines the roots of Russian Marxism in the nineteenth-century radical movement. There is a chapter on George Plekhanov. The remainder treats Lenin and his milieu, including the early Trotsky, and the "empiriocriticists" attacked by Lenin in his philosophical writings.

The formation of prerevolutionary Bolshevism certainly belongs to the history of the Second International. That of the later Soviet Marxism takes up the first five chapters of *The Breakdown.* The main focus is deservedly on Stalinism, although there is a chapter on the later Trotsky. For the rest of the volume, Kolakowski turns his attention to that "unknown dimension," i.e., the Italian and Central European Marxism that to a greater or lesser degree stood opposed to the dominant *and* dissident versions of Marxism-Leninism after Lenin. There are chapters on "Antonio Gramsci: Communist Revisionism," "György Lukács: Reason in the Service of Dogma," Karl Korsch, Lucien Goldmann, the Frankfurt School and Habermas, "Herbert Marcuse: Marxism as a Totalitarian Utopia of the New Left," and "Ernst Bloch: Marxism as a Futuristic Gnosis." In one concluding chapter the author tries to cover: post-Stalin East European dissident Marxism, French versions (e.g., Sartre and Althusser), the New Left, and Maoism. There is a critical epilogue. Each volume contains a short "selective bibliography."

Some general criticisms: First, in the preface to *The Founders,* Kolakowski describes *Main Currents of Marxism* as a "handbook," but also "the history of a doctrine." It is supposed "to include the principal facts that are likely to be of use to anyone seeking an introduction to the subject, whether or not he agrees with my assessment of them." Still later, in the same place, he notes that "another feature that gives the work the character of the handbook is that I have indicated . . . the basic facts showing the connection between the development of the doctrine and its function as political ideology. The whole is a narrative strewn with my glosses." Now, are we being offered a reference work, an intellectual history, or an ideological guide to Marxism's errors? Kolakowski understands that an exhaustive reference work is beyond the capacity of a single scholar. But surely a work on this scale requires an explicit methodological apparatus, one which he fails to provide. If, for example, it is an intellectual history relying on interpretation of texts, then the proposed distinction between "principal facts" and the author's "assessment" thereof is questionable: in interpretations of symbolic structures "facts" have no objectively autonomous status.

Second, as might be expected, from the opening pages on Marx on, *Main Currents* is afforded (although not exclusively) a "philosophical" orientation. This is not illegitimate; but what may have "broken down" is not so much Marxism as *social* theory, or as action-orienting historiography (e.g., Eric Hobsbawm, E. P. Thompson, especially Gramsci in his historiographical writings), but as the philosophical world-view inscribed in different ways by Engels, Stalin, and Lukács. More positively, although no contribution to Marxism is immune to critical review, the renewal of the theory is as palpable as its "breakdown." A misleading portrait of Marxism today is provided by the compression of its post-Stalin history into a single chapter. Precisely in this period theorists from Merleau-Ponty to Rudolf Bahro have deepened our understanding both of capitalism and of what has claimed to oppose it. In the course of their studies they have not only "applied" Marxism but have "reconstructed" it. Such reconstruction, as Habermas has observed, is normal for Marxists.

THEODORE MILLS NORTON

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Philosophy, Politics and Society, 5th ser. Edited by Peter Laslett and James S. Fishkin. (New Haven: Yale University Press, 1979. Pp. 312. $19.50.)

Tyranny and Legitimacy: A Critique of Political Theories. By James S. Fishkin. (Baltimore and London: Johns Hopkins University Press, 1979. Pp. ix + 158. $12.95, cloth; $4.50, paper.)

Since the appearance in 1956 of the initial volume in this series, the pages of *Philosophy, Politics and Society* have offered seminal essays by some of the most prestigious political thinkers of the Anglo-American world. One would not have expected a second, let alone a fifth volume, given that first collection's pronouncement that political theory, as a viable intellectual enterprise, was in a moribund state. That judgment was, indeed, premature, and over the intervening years Peter Laslett, occasionally with collaborators, has presented us a chronicle of changing styles and foci, in political philosophizing. And what changes these 23 years have witnessed! The editors today find the activity not only alive and well, but contrary to Isaiah Berlin's assertion in 1962, they have even felt able to nominate a "commanding work": John Rawls' *A Theory of Justice*.

Doubtless there will be dissent from the awarding of this palm; critics will question the claim to such preeminent influence, as well as the merits of the book itself. Yet, most would, I think, concede the quite remarkable influence Rawls' writings have enjoyed, as in the essays in this collection amply illustrate. To borrow from Thomas Kuhn's protean concept of paradigm functions, Rawls has not only defined a substantive agenda for thinking about politics, and offered a particular principle of distributive justice, but also his book illustrates the process, the "methodology," if one likes, of political theorizing. How should this process or style be characterized? We might call it "analytic" or "formal" as opposed to "institutional," or "historical" or even "substantive." A brief listing of the concerns of some of the contributors (who cannot be considered in any detail in this space) is instructive.

Peter Singer advances an argument for extending aid to deprived peoples, Douglas Rae offers an account of "simple" justice and a critique of Rawls' formulation, Rawls himself contributes an elaboration of a "well-ordered society" flowing from his idea of justice, Robert Dahl and Brian Berry explore the resource of purely procedural conceptions of democracy as opposed to, e.g., meritocracy, Carole Pateman criticizes Hanna Pitkin's formulation of the principle of obligation, Lawrence Kohlberg analyzes the Rawlsian "original position" in terms of "reversibility" or "moral musical chairs," Geoffrey Harrison explores the relationship of rationality to tolerance of behaviors we believe mistaken or malign, and Stanley Benn defends the rationality of participation in macro- or system-level terms. The volume is completed by essays of Robert Lane, Gerald Dworkin, and the editors.

Apparently in anticipation of criticism, the editors write: "Perhaps the reader may think that the complexities of democratic theory as elaborated by our contributors are too refined: The message scarcely plain enough for the political world as it actually is. . . . But this is not the first time in the history of thought when moral perplexity has given rise to refined distinctions and the intricate manipulations of exact definitions (p. 3). On the same page they remark on the absence of writings from "the authoritarian socialist world." These disclaimers are not entirely persuasive. To the former it might be replied that this volume, with the possible exception of Lane's essay on capitalist and socialist characterology, has precious little to say about either politics or society; that indeed the style of theorizing represented here, and in earlier volumes of the series, is indistinguishable from moral philosophy of a more or less utilitarian sort—the sort of philosophizing that dominates the journal *Philosophy and Public Affairs*. The student of *politics* might well be tempted to ask of the contributors the question once addressed to sociological functionalists: "They use the bit and snaffle all right, but where's the bloody horse?" It needs to be added that not only are these essays in democratic theory, but also they represent mainstream Anglo-American philosophy, and the culture and society which supports it. It is scarcely plausible to suggest that all socialist thought is "authoritarian," or that the elaboration of principles of justice, decision, toleration and paternalism are similarly meaningful in Third World politics as in Western societies, and it is at least surprising to encounter no contributor reflecting a Marxist, or a revolutionary posture.

The subtitle of Fishkin's book (a condensed statement of his thesis is contained in the volume discussed above) is misleadingly broad; he is actually concerned to criticize certain characteristic moves made by political theorists in the tradition to which I have alluded earlier, and to offer a criticism of and alternative to *A Theory of Justice*. He is clearly influenced by Robert Dahl's writings on democratic theory, but these foci and indebtedness should not obscure the fact that Fishkin's argument *is* a broad one, applying, in his words, "to the libertarian theory of Robert Noszick, to the liberal theory of John Rawls, and to the socialist proposals of radical egalitarians" (p. 4), and "to majoritarianism as a procedural principle, to utilitarianism, cost-benefit analysis, and welfare economics in general."

Succinctly put, Fishkin asserts "that virtually all of the principles currently prominent in political theory . . . if applied without exceptions or qualifications, . . . would legitimate what is here defined as 'tyranny' " (p. 3). He does not conclude from this assertion that legitimating principles must, perforce, be rejected out of hand, but rather that their application or "spirit" must be modified or weakened, possibly by adapting "non-tyranny" as a priority rule. The reach of his concerns are indicated by the three categories of potentially tyrannous principles he considers "procedural" (majority rule); "structural" (equality); and "absolute rights" (after Noszick's formulation). He regards his essay as the attempt to "represent a new departure, a "systematic proposal of such an agenda for political theory" (p. 122).

But how, we might be moved to ask, are we to understand "tyranny" in Fishkin's sense? In "simple" form, we learn it is a policy choice which "imposes severe deprivations [destruction of an essential interest] even though an alternative policy would have imposed no severe deprivations on anyone" (p. 18). Apart from the conceptual, let alone the empirical, problems of defining severe deprivations and essential interests, one completes this essay with a sense of unfulfillment. The author seems to anticipate this reaction when he notes in his conclusion that it is "so much the better" if his principle of non-tyranny, here articulated in the form "harms—especially serious ones—cannot be justified when they are entirely avoidable for everyone" is found obvious, for he intends no claim either "controversial or surprising" (p. 123).

So be it. The injunction that governments refrain from doing harm when it is entirely avoidable certainly seems minimal as well as obvious, but it is also a near tautology. Fishkin correctly admits that the principle is not helpful in distinguishing among policies where alternatives are not available, nor are many policies of significance likely to leave untouched areas which some will view as "essential" to their life plans. The principle may enable us to distinguish between the holocaust and prohibition (his example), but is it an appropriate strategy to erect non-tyranny into a principle, or are we better advised simply to remain sensitive to the limitations of whatever legitimating principles we adopt, even at the risk of transforming them from formal imperatives to ad hoc recommendations?

PAUL F. KRESS

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Usable Knowledge: Social Science and Social Problem Solving. By Charles E. Lindblom and David K. Cohen. (New Haven, Conn.: Yale University Press, 1979. Pp. viii + 129. $10.00, cloth; $3.95, paper.)

Charles Lindblom and David Cohen have written a challenging book which serves as a useful introduction to political scientists as well as social scientists interested in social problem solving. The book, which represents an extended essay, offers a research program "that we both set for ourselves and commend to our colleagues who study social problem solving, policy making, policy analysis, and the place of social science and social research in them" (p. 3).

The essay sets out to explore the "fundamentals of social science and social research" as instruments of problem solving—specifically, Lindblom and Cohen characterize the phenomena they analyze as *professional social inquiry* (PSI). The authors suggest that social scientists (1) are preoccupied with what they call *analysis* (i.e., information derived from the methods of social science carried out by professionals, and "authoritative" by virtue of its association with science) (pp. 7–8); (2) have an overinflated opinion of their contribution to problem solving: ordinary knowledge and social interactions (negotiation, coin-tossing) make *at least* as great a contribution as PSI. "It is only when PSI and other knowledge are joined in mutual support that knowledge about the social world becomes authoritative" (p. 53). Indeed, Lindblom and Cohen would argue that analysis (at best) augments ordinary knowledge and common sense; (3) have seriously distorted the role of analysis in policy making by conceiving of the social problem-solving process as "scientifically rational" and one in which the contributions of PSI will always be relevant and dominant as opposed to tradition, prejudice, or power (p. 37); and (4) are misguided when (in the name of objectivity in science) they direct individuals away from normative issues (p. 46). If one accepts that regular forms of social interaction do more to solve social problems than PSI does, then it is logical to conclude that analysis should be reoriented towards helping participants in their interactive roles and in diagnosing, evaluating, and improving the very process of social interaction (p. 60).

Essentially, Lindblom and Cohen seem to be making these points: that problem solving is not as rational as academicians in general and social scientists in particular would like it to be, that formal social inquiry has overestimated its contribution to social problem solving, and that, in fact, formal social inquiry *by itself* has made a minimal contribution to social problem solving. Moreover, the authors argue that there are some fundamental problems in the relationship between the social sciences and their translation into concrete policies/programs. These problems relate to the methods governing social inquiry (PSI is incapacitated by "its own metaphysics, fashions, traditions, and taboos" [p. 95]), the context (social structure) in which the creation and application of PSI takes place, and the relative isolation in which social scientists and policy makers have viewed each other; political, economic, and social constraints have not been taken into account.

Unfortunately, this essay does not trace or understand the deep, historical roots of the issues the authors are writing about. The advancement of knowledge and how it is perceived by society (how it fits into the social structure) is an issue of long-standing concern to philosophers, scientists, and heads of state. What Lindblom and Cohen label as PSI, with its inextricable link to the sciences, dates back to the "intellectual revolution" of the seventeenth and eighteenth centuries.

Prior to this so-called "revolution," knowledge had not been divided into several, distinct branches. Sir Francis Bacon was the first to identify "science" as a distinct and critical mode of inquiry leading to the production of knowledge, as opposed to "the arts," as the use of knowledge. Since that time, it has been fiercely debated whether "science" is the only true form of knowledge, a superior form, or simply one form of knowledge among many others (religion, law, philosophy, technology). There has also been much controversy over the extent to which scientific knowledge (as defined by Bacon) is useful in conducting the affairs of state. Since Bacon's time, there were many who linked industrial progress to the advancement of science and technology. Not everyone agreed that science would make a positive contribution to mankind (e.g., Karl Marx). Indeed, one group of philosophers (the humanists) were particularly concerned with the potential destructive and harmful aspects of science. Aldous Huxley's *Brave New World* (1946), for example, warns against the dangers of the pursuit of science unguided by any concern for human values. Lindblom and Cohen would have served their readers well by outlining some of the basic historical roots of the research agenda set out in their work.

It is also worth noting that the notion of adapting knowledge (ordinary knowledge as well as controlled laboratory experiments) dates back to the Greeks and is a theme running through much of Western thought (e.g., the problem of theory and practice). Practitioners of PSI were not the only ones to recognize the need to weave knowledge into the fabric of society. To the great philosopher Auguste Comte, for example, it was evident that changes in social structure are intimately associated with changes in modes of knowing–this is almost the same notion as Lindblom's and Cohen's modes of social interaction.

In the end, we are reminded of a very important point: social problem solving can only be understood in the context of economic, social, and political realities, and the production of knowledge and research should be guided by this reality. This is a point well worth reiterating. However, the book is not warranted in its claim to new ideas. Nor is it warranted in scolding social scientists for "errors" which are discussed in some depth by historians of science, sociologists of knowledge, and, more recently, by those interested in the uses of knowledge.

ROBERT F. RICH

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Psychopolitical Analysis: Selected Writings of Nathan Leites. Edited by Elizabeth Wirth Marvick. (New York: John Wiley, 1977. Pp. 384. $21.95.)

In 1979, the International Society for the Study of Political Psychology gave Nathan Leites its Harold D. Lasswell award for Leites' career of prolific and brilliant contributions to the study of psychological aspects of politics. Many who attended the award ceremony commented, as Elizabeth Marvick does in her introduction to this collection of 18 carefully selected and intelligently condensed excerpts from his *oeuvre*, on the inverse relationship between the quality of his work and the attention it receives.

Leites' writings have always been controversial, are little read today by political scientists, and so far have not had a major impact on their discipline. This is so although his copious published and unpublished (but extensively circulated) writings crackle with original insights and exhibit uniquely rich confluences of perspective from many disciplines and deep erudition on such diverse matters as politics, culture and society in France, Russia, China, and Germany; interpretation of political motivation, belief and psychology; psychoanalytic theory; logic of science; philosophy; and even analyses of motion pictures.

Ranging eclectically through his writings (the editor wisely presents a carefully selected rather than exhaustive Leites bibliography) one thinks of such diverse works as the dazzling case study of a Fourth Republic French presidential selection (*The House Without Windows*), which is replete with an index that uses one notation system to indicate propositions explicitly imbedded in the study and another to indicate implicit propositions; a voluminous work in which emotional themes in passages from the writings of Lenin and Stalin and from nineteenth-century Russian fiction are juxtaposed (*A Study of Bolshevism*); one of the most rigorous and closely argued statements ever made of the logic of validating "psychopolitical analysis of political behavior" (his article of that title, which appeared in *World Politics* and is condensed in this volume); and writings on the psychopolitical implications of existentialist philosophy and fiction.

Leites' work has been consistently informed by psychoanalytic theory, even when, as in *The Game of Politics in France,* he does not use psychoanalytic terminology. Being a mundane lot, wedded to institutional and straightforward cognitive psychological explanations of politics, most political scientists have found his work *recherché*. Further, although his meta-observations about the logic of inquiry tend to be starkly positivistic, his own empirical writing seems incorrigibly "subjective." (Often he issues a rigorous caveat about the provisional empirical standing of what he is about to say. Then he soars imaginatively. Readers forget the caveat.) Finally, I suspect that he writes too well for most of us. His prose is that of the literary intellectual, not the stolid scientist.

Elizabeth Marvick properly points out that the diversity of Leites' contributions defy brief summary. Hence she stresses that the passages she has selected and her introductions to them illustrate rather than adumbrate the full range of his thought. The selections in this book are excellent. They include unpublished writings and writings that have never appeared before in English. The reader who takes times to mine this welcome anthology, including the editor's judicious introduction, will emerge with hypotheses enough for decades of writing and research. Along the way that reader will have savored prose of an aesthetic quality political scientists rarely encounter in their professional capacity. Finally, many of Leites' concerns—violence, terror, disorders of affect, selective perception, for example—are among the profoundly disturbing themes of our epoch. Hence the book is an effective antidote for complacency.

FRED I. GREENSTEIN

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Systémismes et Politique. By Anastase-Jean Metaxas. (Paris: Editions Anthropos, 1979. Pp. 248. Price not listed.)

This is an analysis of the "systemic conceptualizations" of David Easton and Gabriel Almond in the light of the systems approaches of Pareto and Parsons. The analysis flows from Metaxas' conception of an emerging political/social reality he terms "holistic." Metaxas' general conclusion is that, for all their undeniable value in moving us toward a reconceptualization of the political that will take into account the insights of recent scientific thought and of concrete social experience, these analyses by Easton and Almond are not what we are looking for to guide us into the post-modern world.

Metaxas finds Easton and Almond trapped in a "monodimensionalism" that fails to take into account adequately tensions in society between order and counter-order, among, one might say, representations of the dialectic. Such a perspective, he feels, misses the drive of social units toward true autonomy within a suffusing and supporting participatory macro-environment. It ignores the existence of the autonomous political as a necessity in the life of a self-fulfilling person. The Easton/Almond prison, Metaxas suggests, is an American provincialism. Easton's system ends up elitist; Almond's exaggerates a dubious Anglo-American rationality as a standard and goal of the evolutionary process.

This is a provocative work. There are rich insights illuminating careful scholarship. Metaxas raises questions which are really doors of exploration in our understanding of political conceptualization.

But the highly abstract quality of the analysis becomes a problem for further development. Elaboration of the "holistic" structure and process would seem to require an immersion of the Metaxas model in the concrete action of complex political/social systems of our times. Evaluating Almond and Easton demands a feel for the context of the action which produced them and which they produce. The task is to objectify the interpenetration of theory and fact that makes theory a creative instrument.

American and Western European institutions alike seem to be under revolutionary impact. Analyses such as that of Metaxas can achieve the force of value if they succeed in tapping and translating into communicable ideas the substance of practices and their symbolization that in mature systems such as the American and Western European prefigure the person-oriented societies we are capable of producing. The Nazi experience was as near to a holistic experience as Europeans have known. Constitutional theory must define structures resisting the formation of such a pathological condition in the future.

Perhaps Easton and Almond have a problem in failing more fully to objectify the spirit, atmosphere, and texture with which the American empirical is saturated. But, provocative as Metaxas is, his analysis tends toward a unidimensional quality by prescinding from the complexities, nuances and rhythmic sensitivities of Americans in particular as political persons. In this respect, Metaxas would, I think, find very useful Alexis de Tocqueville's analysis of the American tyranny of the majority and James Madison's concept of political personality in Federalist 10.

ALBERT A. MAVRINAC

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Methodological Approaches to Social Sciences. By Ian I. Mitroff and Ralph H. Kilmann. (San Francisco: Jossey-Bass, 1978. Pp. xv + 150. $11.95.)

The central arguments of this book fit into and carry forward recent trends in the "greening" of science. The first chapter puts the psychological, sociological, and logical dimensions of science on an equal footing. More orthodox philosophy of science subordinates the psychological and sociological to the logical. Who does science and where is not important; how statements relate to theories and correspond to structured observations is decisive. The perspective of the authors is that styles and norms are equal to logics which are simply preferred.

The book is interesting. It stimulates reflection about what one does as a social scientist and how one evaluates colleagues. It is also important. Scientific progress requires self-scrutiny, at the very least to identify pathological practices. This is one diagnostic approach.

From various studies of the behavior of scientists and from what scientists write about themselves, a typology of four types of scientists is developed with concepts taken primarily from Carl Jung. Each is called a methodology (although the terms "styles" of "inquiry" used in the first chapter are more descriptive). Each is defined according to style of inquiry, logic, and sociological norms or ideology.

The Analytical Scientist is the modal empiricist, taken to experimental methods of testing hypotheses with little concern of how they are formed, and impatient with conjecture. The Conceptual Theorist, the most prestigious, wants to create new theories and interpretations, challenges accepted ideas, is willing to ignore data that contradict the theories, but defers to research as the ultimate arbiter. The Conceptual Humanist shares with the Theorist a macro-theoretical orientation, but believes that science is not value-free, indeed, that it is political. The Particular Humanist sees science as only one of many ways of knowing, recognizes its value content, is oriented to the logic of the unique, and consequently is dedicated to in-depth case studies. The final chapter tries to put these types of activities together in two "holistic or systemic models" (p. 106).

The authors' style is modest, open, and therapeutic. They lean a little heavily on the Analytical Scientists and those with that style might find their characterization simplistic. And for those who believe that science has ontological status independent of scientists and society, what is said will be treated as insightful and perhaps useful but not serious.

Although the greening or softening of our views of what we are doing is more honest and counters the bias of experimental methods toward trivialization, it also is a danger to the development of science. The norms of science do not sit well with those of the modern state which encourages or tolerates buying, threatening, bluffing, lying, subordinating, and asserting certitudes. The niche that slowly evolved for scientists and their institutions, with their bias toward truth in the long run, is precarious. When those that occupy that niche openly question their beliefs, especially that at least there are methods for ascertaining truth which can be useful, invasion by others is invited. The dilemma is to uphold our norm of honesty, including application to ourselves, and to maintain the integrity of that delicate little niche that holds the heritage of intellectual achievements.

HENRY TEUNE

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Women in Western Political Thought. By Susan Moller Okin. (Princeton, N.J.: Princeton University Press, 1979. Pp. 371. $22.50, cloth; $4.95, paper.)

It has always been apparent to those who cared to notice that the place political theorists have usually assigned to women is much beneath the place assigned to men. As Susan Okin notes in this pointed, thoughtful book, even Plato, though he included women among the guardians, thought men would much outnumber them at the upper ranks. And J. S. Mill, despite his feminism, thought women, once they married, should stay home. Why have political theorists, with the possible exception of some socialists, so constantly resisted equality for women? This is the main question Susan Okin raises. Her method is analysis of the arguments about women advanced by Plato, Aristotle, Rousseau and Mill. Her findings reveal our theoretical tradition to be vexed by alarming conflicts and lapses that have generally been evaded or ignored.

The crucial premise in the argument of all four theorists for giving women a low social place is identified by Okin as the claim that within families the husband should wield authority, while the wife should raise children and keep house. Women will not reliably fill their subordinate domestic role, these theorists claim, if they are men's social equals. A women who is on a par with men in prestige, wealth or power may not be willing to make a home, let alone obey her spouse.

This argument is deployed differently by each of the four theorists, and Okin is ingenious in tracing out its variations. It is most baldly advanced by Aristotle, who does not even consider the possibility of treating women the same as men. So convinced is he of the need for women to be subordinate in families that he makes inferiority part of their social nature. Plato's version of the argument is more nuanced. He too held, though less tenaciously than Aristotle, that women inside families should raise children and obey. They enjoyed social equality in *The Republic*, because in the regime there envisaged the lack of families meant that the question of women's domestic place did not arise. But since families are essential to the regime planned in *The Laws*, Plato there recommended, out of concern for domestic order, that women have a subordinate social status. Rousseau, being a partisan of social and political equality, does not draw out as explicitly as do Aristotle or Plato the inegalitarian social implications of their shared insistence on giving women a subordinate domestic role. Yet Okin shows how by his silences, by his praise for Geneva's sex-segregated clubs, and, above all, by his views on female education, Rousseau opts implicitly for confining citizenship in his ideal democracy to men. Finally, J. S. Mill, because he denies men domestic authority, concludes that in the social sphere women have a right at least to equal status. But his unwillingness to relieve married women of domestic and parental duties prevents him from allowing them completely equal social treatment.

By tracing her theorists' arguments for giving women a low social status to the claim that they should be subordinate in families, Okin shows that unless this claim can be established, their case for sexual inequality fails. Everything she says about why her theorists make this claim casts doubt on its merit. Plato, for instance, is portrayed by Okin as giving women a subordinate domestic position in *The Laws* out of a surprisingly uncritical acceptance of tradition. And she finds Rousseau's espousal of domestic subservience for women to spring mainly from neurotic fear of their sexual ascendancy and from parochial concern, verging on obsession, to assure husbands paternity of the children born by their wives. Having revealed her theorists' claim for women's domestic subservience to rest on these flimsy considerations, and on others that are just as insubstantial, Okin is persuasive when she berates their case for female inequality as groundless and as compromising the integrity of their thought. For political theorists to have relied on quirks and prejudice to justify subordinating half the human race is surely a great intellectual scandal.

A rereading of these four theorists in the light of Okin's charges might possibly lead to their partial exoneration. This seems most likely in the case of Rousseau, who sometimes claims to favor domestic equality for women, who gives some arguments for their social subordination based on political, not familial concerns, and whose ideal of citizenship, since he illustrates it with the example of a selfless Spartan mother, may possibly include women. One might also find that Rousseau, in espousing female inequality, was prescribing another in his long list of distasteful yet ineffective remedies for the corruption that he thought had ruined modern social life.

But even if some of the arguments for female inequality examined by Okin should turn out to be better grounded in reasoning than she suggests, this need not make them acceptable, since a reasoned argument may still be unconvincing. Political theory is bound to grow in depth and power, if those who face its problems learn the message of Okin's book. For the case for female inequality will then be revealed as so doubtful that unless the presumption against it can be somehow overcome, it will have to be definitively rejected.

ALAN RITTER

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Compromise in Ethics, Law, and Politics: Nomos XXI. Edited by J. Roland Pennock and John W. Chapman. (New York: New York University Press, 1979. Pp. xiv + 212. $17.50.)

Here is a book offering an exhaustive account of compromise in public affairs. With its breadth and its attendant bibliography, it is a fine source for anyone wishing to ponder or recall the dilemmas of compromising and being compromised. It even ends on the intriguing note that our sense of ethics (and, conversely, our indignation at injustice) may be the distillate of a long chain of necessary compromises.

"Compromise" is simultaneously a dirty word and a mature tactic. Perhaps it is not by accident that this volume was conceived at a convention of law professors. It may be the child of late-night plea bargaining. In any case, when one is faced with a book put together by two editors and 11 authors, the better part of valor is to comment on the overall compromise that is the book.

The pure meaning of "compromise" is found in its Latin root, *mittere*, to let go; thence, for the parties together to let a dispute go into arbitration (L. compromittere). This crumb of etymology reveals the hidden role of coercion and fate in compromises: either third parties impose a compromise, or nature does. Logically, if the adversaries were simply capable of reaching compromise through "concessions" or "mutual respect" (words overworked in this book), compromise procedures would not have had to be instituted in the first place.

The very existence of such procedures suggests already the bane of stalemate. A stalemate, by definition, occurs when one or more of two or more adversaries cannot abide the status quo. Hence, "compromise" at root refers to whatever power is invoked (or invokes itself) to break a stalemate. A secret agent is said to be compromised when a third party interposes between principal and agent. In the law, a party is "stultified" by allowing an inconsistency into his or her argument. By way of contrast, the notion of *composing* a quarrel (L. *componere*, to put things back together) leave more to the imagination—since the parties themselves, or time, or any number of agents, could do that.

Being in the analytic tradition, this book does not always appeal to common sense. It informs us, as it were, that King Solomon would not have been well advised to cut the child in two. Too often its fashionable academic pose takes the direction of an argumentum ad ignorantium: "I (you) do not know what is best, *therefore*, compromise is best." Insofar as the authors drift toward this fallacy, and insofar as they perpetuate unargued the liberal claim that values are subjective, talking up "the spirit of compromise" and talking down the winner-take-all mentality, they begin to sound unprincipled by reason of evasion.

The ideology of pluralism—which would have us believe that compromise is, on balance, a good thing—is precisely the ideology that needs to forget that, of necessity and by definition, all compromises are to some degree imposed. Some of the authors admit this by distinguishing sharply between compromise, negotiation, and bargaining.

One of the ideological charms of compromise—i.e., of treating it as if a virtue—is called "buying time." Usually status quo interests win on that move. So it almost goes without saying that interest in compromise, conciliation, arms control, peace research, and the like, fluctuates with perceived advantage. We may recall the uproar in Congress occasioned by Paul Kecskemeti's RAND study, *Strategic Surrender* (1958), and compare that to the attraction, to some of the same people, of Liddell-Hart's theories about tactical retreat and blitzkrieg.

Closer to home, if political science is closer to home, is the realization that the nation-state has always and everywhere been an historic compromise. The real-world complexity of politics thus calls into question any analysis grounded on self-interest versus mutual respect, or individualism versus altruism—as if ethics were a counterfactual "science" of wishes.

Another oversight, and one which does give this book an air of political naivete, turns on the aforementioned sense of compromise as something forced on someone. The obverse, of course, is that the dominant class always holds up compromise as proof of "unity." It is true that we all long for the consensus that never was. It is also true that Caesar, whose foot rested gently on the necks of others, may even have believed that the fictive unity is established fact. Furthermore, political, legal, and (arguably) most hard-core ethical choices must be made in historical time. That is, they are made against what is appropriately called a "deadline." And they are in important measure specific to their own historical context. The question, then, is whether political science *ought* to bother with de-politicized abstractions.

A more interesting question for this book would have been the empirical one, whether people disposed to compromise have suffered more, less, or about as much as, intransigents. The bedrock question, also passed over here (but cf. 194 ff.) is the Platonic one: what do those who do not understand the doom of justice *have* to concede or bargain away? In other words, the issue is tactics. Tactics means nothing divorced from the values in conflict. Hence, abstract musings over procedural conventions, adrift of grim historical fact, remind us of those old stories of public executions that went into too much detail about who of the town was present, what they wore, and said, and whether the victim soiled himself.

Outside its specific historical context, there is no compromise for a reasonable person to praise or blame. Here even Hobbes, the bringer of "science" to the study of politics, mis-spoke himself when he wrote categorically, "There is scarce a Common-wealth in the world, whose beginnings can in conscience be justified."

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The Reveries of the Solitary Walker. By Jean-Jacques Rousseau. Translated by Charles E. Butterworth. (New York: New York University Press, 1979. Pp. xix + 268. $17.50.)

Neglected for half a century by English translators, Jean-Jacques Rousseau's last work, *The Reveries of the Solitary Walker,* has not received the widespread and serious attention it deserves from political theorists. The *Reveries* is revealing on a number of fundamental issues of concern to Rousseau, including the nature of the self and of ultimate happiness, the relation between morality and lying and the truth of religion. These reflections carry political implications. Rousseau calls on philosophers to censor their public statements and criticizes Montesquieu in this regard; he draws distinctions between the "natural man" and the citizen, and records his final views on the situation of a good and wise man in a corrupt world. The *Reveries'* undue neglect stems in part from the form of the work: it is a highly personal autobiography, which Rousseau claims was written for himself alone. Even more problematic, many of the arguments are conveyed only implicitly, their sense sometimes further obscured by Rousseau's distracting complaints that others plot against him.

With the publication of this volume, Charles Butterworth makes two important contributions to Rousseau studies. First, he provides a complete English translation of the *Reveries* that is both accurate and graceful. Butterworth's translation is superior to the only one available in recent decades, the version by John Gould Fletcher, originally published in 1927 and reprinted by Burt Franklin in 1971. Fletcher's translation suffers from a number of distortions and a stylistic unevenness that at worst reduces Rousseau's prose to awkward and stilted phrasing. Butterworth scrupulously avoids the temptation to weave his interpretation into the translation. Claiming accuracy as his first priority, Butterworth achieves a translation that is reliable throughout, without sacrificing too much of the lyrical quality of the original text. Coincidentally, another new English translation of the *Reveries* was published last year by Peter France (Penguin, 1979). Printed in paperback, the France translation's main advantage is its low price ($2.95); it is slightly less accurate, though occasionally more readable—France eschews literalness for attempts at "music and eloquence"—and is based on much less thorough scholarship. France's translation is accompanied by a cursory introduction, while Butterworth's is bound with an array of carefully researched supplements: preface, notes, interpretative essay, two appendices and an index.

In the preface, Butterworth briefly indicates his view of the significance of the *Reveries* and presents a short account of Rousseau's life. Many of the notes offer useful clarification. For example, when Rousseau discusses Mme. d'Ormoy and her novel in the second "Promenade," Butterworth provides a synopsis of her book and relevant quotations from it. Similarly, when Rousseau criticizes d'Alembert's eulogy of Mme. Geoffrin in the ninth "Walk," we are supplied the passage in question. Butterworth is remarkably thorough: some notes even supplement Rousseau's Latin botanical references with common plant names while others retrace his walks with modern-day street names. The first of the appendices contains a description of the Neuchatel manuscript on which Butterworth's translation is based; the second is a translation of that part of Rousseau's *Confessions* which parallels the fifth "Promenade" of the *Reveries* and its account of Rousseau's stay on the Isle de Saint-Pierre.

Aside from the translation itself, Butterworth's long interpretative essay also stands as a valuable contribution. The essay contains a detailed and sympathetic reading of the text which sets forth many of the basic questions raised by the *Reveries.* Written in a style accessible to a variety of audiences, it is a useful antidote to the many reductionist views of this work which focus on Rousseau's alleged paranoia as the mainspring of interpretation. Though readers well versed in the scholarship that exists in French on the *Reveries* may find themselves on somewhat familiar ground, Butterworth's essay is provocative. It should make the *Reveries* more interesting fare to political theorists and stir debate over the meaning of Rousseau's last work.

The essay begins with a discussion of how the *Reveries* compares with Rousseau's earlier works, particularly the autobiographical writings. As Butterworth points out, Rousseau suggests that the *Reveries* is written as a sequel to the *Confessions,* not the *Dialogues.* The essay then proceeds to an analysis of each of the ten sections or "walks" of the *Reveries.* The most controversial claim in Butterworth's interpretation is his view that Rousseau here reveals his earlier religious teachings to be "pious lies" told to "combat his contemporaries and defend the faith of simple citizens" (p. 181). Rousseau's ultimate happiness, described in the fifth promenade, is not religious; rather, as Butterworth emphasizes, it consists in feeling "the sentiment of existence," a radically asocial experience not available to most human beings who live in society and must work. One of the strengths of Butterworth's interpretation is his sensitivity to the connections which link one promenade to the next. While he presents his view of the significance of the order of the third, fourth and fifth promenades less boldly here than in an earlier paper he presented at the 1975 meeting of the Midwest Political Science Association, he adds new insights dealing, for instance, with the importance of the principles of egalitarianism and mutual contentment in both the sixth and ninth walks. There are some specific points I would take issue with in this essay: for example, Butterworth's characterization of Emile as citizen (p. 179) and his labeling of the *Confessions* as "superficial" (p. 149), an assessment which underestimates the moral intention of that work. Also, several parts of the analysis, such as the comparison with Montaigne, could bear development. Another shortcoming of this volume is that it contains no bibliography, annotated or not. On the contrary, Butterworth seems to go out of his way in both the essay and notes to avoid all reference to secondary sources. Still, this is an important book, one to be set alongside Allan Bloom's recent translation of *Emile* and the

Masters' translations of the *Discourses* and *Social Contract.* It is my hope that the next addition to this updated shelf of translations will be an unabridged rendering of *La Nouvelle Héloïse.*

MARY L. BELLHOUSE

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The Idea of Freedom: Essays in Honour of Isaiah Berlin. Edited by Alan Ryan. (New York: Oxford University Press, 1979. Pp. vi + 297. $15.95.)

This outstanding collection of essays, perhaps one of the best works of its kind, is a fitting tribute to Isaiah Berlin, whose contribution to our understanding of ourselves and of our liberal tradition of politics places him among the great thinkers of this century. The essays collected here are of consistently high quality and of great scope, addressing literary, historical, and metaphysical problems, in addition to concerns central to political philosophy. The volume, which includes essays by James Joll, Arnaldo Momigliano, Stuart Hampshire, and Franco Venturi, ends with a thorough bibliography of Berlin's writings by Henry Hardy which will place American researchers in his debt for years to come. The value of this volume for those interested in political theory will be found in the philosophical discussions of the issues raised by Berlin's defense of a conception of liberty on the bases of (1) a pluralistic view of human values and (2) a view of our nature as purposive beings.

It seems reasonable to begin with Bernard Williams' probing article which questions the doctrine of "incommensurability" in Berlin's account of pluralism. Ethical certainty continues to elude us, says Berlin, because we have no single, hierarchical scale on which to arrange our conflicting values. But, argues Williams, because a trivial gain in equality at the expense of an enormous sacrifice of liberty could not be justified to any pluralist at all concerned with liberty, the incommensurability claim either is false or must be qualified. If Williams is correct, then it is possible that Berlin might be too skeptical about the resolution of moral conflicts.

Richard Wollheim's article also addresses Berlin's pluralism, in the context of Berlin's claim that J. S. Mill abandoned utilitarianism. Wollheim reasons that Berlin was attracted by Mill's pluralism, but repelled by the apparent monism of utilitarianism; hence, Berlin must argue as he does that Mill rejected his father's utilitarianism. Yet Wollheim sees Mill as advancing a new and more vigorous, because pluralistic, version of utilitarianism. Reasonable though this seems, Berlin's exploration of Mill's utilitarianism is still more penetrating; Mill did not "revise" utilitarianism because it was monistic, but because it did not square with his view of human nature. Not simply a rights-based theorist masquerading as a utilitarian, Mill coherently sought a new ethic to express what he saw as our obligations, given that we were not merely rational calculators of interest and advantage but also beings who live for purposes. Thus, insofar as Mill rejected his father's conception of human nature, so he must abandon utilitarianism. This new conception of human nature explains not only Mill's pluralism, but also his dedication to rights and liberties—for if we are indeed purposive beings, then we must construct a system of rights to preserve our humanity.

This concern with the rights which protect individuals in their choices is the heart of liberalism. And H. L. A. Hart is exactly right to worry about the new critique of utilitarianism by deontological theorists. While Hart is convinced that this critique is well founded, his article aims to demonstrate why we should be dissatisfied with the existing efforts to provide a reasonable alternative. First he criticizes Robert Nozick's account of rights, understood as "moral side-constraints" on action, as ignoring other human needs—thereby endangering the rights it justifies. Hart then criticizes Ronald Dworkin for attributing political evils to the violation of the *equality* of respect and concern; for, more accurately, political ills are due to violations of the liberty of, or respect for, persons. In short, both of these theories suffer for failing to address the *human* dimension of liberty. Yet, if Berlin is right about Mill (and Sorel), then who and what we are is the central concern of political theory.

It is in this spirit that Peter Gay aims to explore further a non-rationalistic understanding of our humanity in his engaging essay on the complexity of Freud's psychological theory. Yet, also in this spirit, Charles Taylor insists that a human account of liberty requires a more positive emphasis than Berlin would countenance. Unless, in emphasizing the "opportunity-concept," we are to reduce human liberty to the mere absence of restraints like traffic lights (and so to count Albania a freer country than England!), we must humanize liberty by introducing considerations of the "exercise-concept" of positive liberty. Only in this way can we hold claim to much of the most inspiring terrain of liberalism, negatively conceded to liberty's opponents.

Finally, this concern with our rights inevitably gives rise to questions about the freedom of the human will. Berlin suggests that (for a conceptual reason of some unspecified sort) we cannot make sense of claims which ascribe guilt or credit to agents who faced no choice in acting. Although "ought implies can," the nature of the relationship between "oughts" and "cans" is not one of logical implication. In an extremely challenging and thought-provoking article, Morton White argues persuasively that the relation between "oughts" and "cans" is itself moral in nature: we ought not (morally) to blame someone for doing something when there was no choice. Thus, " 'ought' ought to imply 'can' " is more accurate than "ought implies can." After presenting his case, White concludes (perhaps lamely) that he and Berlin should continue to think and communicate about this issue. Yet, if generalized, this might be the best advice we could follow—we would all do well to continue to think, and communicate, about the issues raised by Berlin's compelling account of free human action in a cosmos not structured by an eternal plan. Alan Ryan and his contributors have only begun this task, but they have done so in an admirable and fitting way.

ROBERT A. KOCIS

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Quest for Equality in Freedom. By Francis M. Wilhoit. (New Brunswick, N.J.: Transaction Books, 1979. Pp. xii + 281. $12.95.)

Francis Wilhoit's intention is to describe and analyze the "origins, ideological tensions, and public policy ramifications" of "the fundamental conflict between liberal and egalitarian values" in modern constitutional democracy—especially the United States. His purpose is to make a substantive contribution to "the discussion of equality in political philosophy and to the analysis of political controversies that involve the issues of freedom and equality." At the same time he wants to write a book for non-specialists as well as scholars which links "the great battles over freedom and equality with the specific circumstances, historical and sociopolitical, in which they arose" (p. xi). All of this is indeed a worthy and ambitious undertaking.

The body of the book contains an introductory chapter surveying theories of equality in Western thought from the Old Testament to the present. It is followed by chapters tracing the development of the struggle for equality in political participation, in criminal justice, for economic, racial, ethnic, and sexual equality and for equality of opportunity in education. The final chapter contains the author's prognostications about the future of the struggle between egalitarians and conservatives and its consequences for the political future of the United States.

There is much that is of interest in the book and it is presented in a manner free of the jargon of specialists and thus accessible to the student and the lay person. The chapter on the quest for socioeconomic equality, for example, presents a lucid, if somewhat oversimplified, explanation of the conflict between pluralists and power elite theorists. This chapter also contains a brief but clear analysis of the origins and demise of the war on poverty. In spite of these strong points, and there are others, the author fails to achieve his purposes because (1) he is confused about and fails to clarify the nature of the problem of equality and its relation to freedom and (2) because he tries to fit controversies into the freedom equality framework which do not raise that issue at all or do so only tangentially.

The author's confusion about equality is illustrated by his discussion of the concept in the Old Testament. He equates "equality" with "equality before the law." The Hebrews, he says, "naturally assumed that all persons, whatever their differences, were equal before the law. Equality in other words, was the ideal norm and inequality a deviation that needed justification. This does not, of course, rule out the concept of hierarchy; it merely required that hierarchical distinctions be rational and clearly legitimated" (p. 11). A few paragraphs later, he writes, "The toleration of slavery, like the acceptance of male supremacy, was a limitation on the Hebraic theory and practice of equality" (p. 12). Now, slavery and male supremacy are "hierarchical distinctions." If they are "rational and clearly legitimated" they are not contrary to equality before the law or contrary to equality. The question then is, "are slavery and male supremacy rational?" That question requires some argument. For example, one might examine the discussion of slavery in Aristotle, *Politics* (1953b15 f.), a discussion crucial to an understanding of the problem of equality which is not mentioned by the author. Or one might examine the discussion of male and female roles in society in Rousseau's *Emile* (Bk. 5) which is also ignored by the author. Wilhoit must either assume that hierarchy and equality are incompatible—contrary to his statement—or he assumes that all such rational basing and legitimating is ultimately arbitrary—i.e., a function of ideological commitment or propensity and not subject to serious philosophical inquiry. Because he makes one or both of these assumptions, he cannot make a contribution to the discussion of equality in political philosophy.

The author's attempt to pack everything into the struggle for equality framework is illustrated by his inclusion of the capital punishment controversy in the chapter on equality and criminal justice, and the inclusion of the controversies over religion and the public schools in the chapter on equal educational opportunity. There is no question that these have been and are important controversies for the American body politic, but they are not controversies primarily arising out of the struggle for equality. It is true that the Supreme Court struck down the death penalty on equal protection grounds in *Furman* v. *Georgia* but the primary issue in the capital punishment cases is not equality in criminal justice, but rather the efficacy and propriety of the death penalty. By forcing the question into his framework of equality, Wilhoit misleads his readers about the problem of capital punishment and about the problem of equality. The conflict over capital punishment is quite simply not a conflict between egalitarians and libertarians. Nor is the conflict over education and religion. The chapter on equal educational opportunity presents the Supreme Court's decisions relating to religion and the schools as conflicts between conservatives and egalitarians. Thus the author tells us that "Protestant as well as Catholic egalitarians agreed with" the Court's decision in *Pierce* v. *Society of Sisters. Wisconsin* v. *Yoder* "gave a boost to the equality claim of all U.S. minorities." *McCollum* v. *Board of Education* "disappointed religious conservatives." *Zorach* v. *Clausen* "shocked religious liberals" and the Bible reading and prayer cases "infuriated Catholic conservatives along with Protestant fundamentalists." Wilhoit concludes this discussion with the comment, "Both secularists and religious conservatives maintain that they suffer a diminution of freedom as well as equality when the Supreme Court rules against them. And like all litigants, they demand absolute justice. In any event, the ideals of both freedom and equality are intimately involved in the controversy" (p. 230). That freedom is involved is not in question. But the issue of equality is raised only tangentially in these cases. In any event, Wilhoit has not shown how, or even that it is involved. Consequently his discussion of these cases, rather than contributing to a clarification of the concepts of equality and freedom serves only to mislead and misdirect the thinking of the non-specialists among his readers. It certainly does not contribute to the "discussion of equality in political philosophy."

KLAUS H. HEBERLE

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American Politics

Financing the 1976 Election. By Herbert E. Alexander. (Washington, D.C.: Congressional Quarterly, Inc., 1979. Pp. xxii + 871. $35.00.)

If money truly is the mother's milk of politics, then the 1976 presidential election campaign is particularly noteworthy, for it represents the first time that presidential aspirants were invited to suckle directly at the public breast. Reacting to the Nixon reelection campaign's creepy fund-raising schemes and to other nefarious activities associated with the Watergate scandals, Congress bestirred itself in 1974 to amend federal election and revenue laws to provide full optional public funding of presidential general elections, optional public funding of national party nominating conventions, and matching public funds for presidential primaries up to a limit of $5 million per candidate. Congress also amended the rules and regulations for raising and spending election funds from private sources. *Financing the 1976 Election,* the fifth of a quadrennial series of studies authored by Herbert Alexander, attempts to develop a comprehensive description and assessment of the impact of these provisions and the amendments to them passed in 1976 in response to *Buckley* v. *Valeo.*

The book itself is really the product of a team effort led by Alexander in his capacity as director of the Citizens' Research Foundation. As a result, it has both the positive and the negative features of a research report of an investigating commission assigned to analyze a complex problem and to present its findings on or before a certain date.

On the positive side, the book is certainly comprehensive. Indeed, its title belies the true time span it covers. Besides providing necessary summaries of the Federal Election Campaign and Revenue Acts of 1971, Alexander and his associates begin their detailed descriptions with developments as early as the opposition of House Administration Committee Chairman Wayne Hays to the 1974 amendments and as late as the January 1979 resolution of the Socialist Workers Party's suit for special exemption from disclosing the names of its contributors.

The breadth of coverage is also impressive. Among other things, the book details the formation and operation of the Federal Election Commission; the pre- and post-convention financing of the presidential aspirants of both major parties, of Eugene McCarthy, and of nearly every significant minor party; the contributions and impacts of the proliferating number of political action committees; the activities and impacts of Common Cause; the costs and impacts of mass media exposure; the effects of the new laws on the conduct of congressional campaigns; the legal battles concerning interpretation and implementation of the new election laws; and the aftermath of their enforcement. Moreover, as behooves the publisher, the entire contents are as thoroughly indexed as a *Congressional Quarterly Almanac.*

In short, the book contains information on just about everything one would like to know about financing federal elections in 1976, save comprehensive lists of all contributors or of all expenditures for political broadcasts. Neither the Federal Election Commission nor the Federal Communications Commission compiled such lists, and the Citizens' Research Foundation found the task too costly to undertake.

On the negative side, the book reads as though it were the product of a committee. Although each chapter is well drafted, the chapters do not hang together well. No unifying argument or theory is developed. The facts are presented one after another with little explicit regard for their importance. This would matter little if Alexander intended the book as a compendium of facts like the *Statistical Abstract,* but the analytic tone of the introduction, and the snippets of theory and bits of analysis sprinkled throughout the text suggest that he intended something more interpretive. As things stand, however, the chapters which form the bulk of the text (essentially chapters 3–10) do not build upon one another; instead, with minor editing, they are virtually interchangeable in order.

In addition, there are two minor irritations. First, the text is replete with unfamiliar abbreviations of the titles of laws and of organizations, yet the book contains no table of abbreviations. Second, as the richly detailed chapters are not arranged chronologically, the absence of a chronology of events makes it harder for readers to discern how developments followed upon one another.

In sum, *Financing the 1976 Election* is a yeoman effort that succeeds as a descriptive study of the formation and impact of federal election laws in the mid-1970s, but it does not invite a reading from cover to cover. It probably will be most useful as a standard source book on election finance, into which scholars and general readers will want to dip for details. And while more satisfactory interpretations of what the details amount to remain to be written, Herbert Alexander and his associates have laid much of the groundwork necessary for writing such interpretations.

MICHAEL MARGOLIS

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Fiscal Retrenchment and Urban Policy. Edited by John P. Blair and David Nachmias. (Beverly Hills, Calif.: Sage Publications, 1979. Pp. 310. $25.00, cloth; $8.95, paper.)

Confronted with a future of slowly growing public revenues and unremitting demands for existing and expanded governmental services, the nation's central cities are entering an era of fiscal retrenchment during which tight budgets will force urban officials to make hard choices among competing governmental priorities and programs. In their introduction to this timely subject, John P. Blair and David Nachmias offer an incisive overview of the causes and implications of retrenchment. As they observe, a sluggish economy, population losses, the creeping expansion of state and federal aid, and the tax revolt have undercut the growth of cities' revenues. Yet central cities face the persistent high costs of services to the heavy concentrations of poor and elderly, the insistent claims of militant municipal unions, and unavoidable expenditures for replacing deteriorating capital stock. Referring to the work of such scholars as Galbraith, Friedman, and Banfield, Blair and Nachmias note that the consequences of retrenchment for good or ill depend upon the perspective from which it is assessed. Indeed, the contribution by Louis H. Masotti argues that past governmental responses to urban problems have led to "programmatic, bureaucratic, institutionalized policy failures" (p. 55); retrenchment, in Masotti's view, may force policy makers to respond more creatively and more effectively to the plight of the cities.

This collection of 14 articles is divided into three parts. The first presents four selections dealing with theoretical considerations and includes the articles by the editors and Masotti. The second section contains four articles on the tax revolt, the most useful of which is John L. Mikesell's comprehensive review and analysis of tax and spending limitation referenda on state ballots in 1978. The third part considers the consequences of retrenchment for such policy concerns as minority economic progress, education, and health care.

Although in general the contributors offer competent and occasionally provocative assessments of various aspects of urban retrenchment, the volume as a whole provides a disappointing and incomplete examination of the subject. For example, the section on the tax revolt deals with the issues largely in terms of public opinion. Missing is specific investigation of the economic and political impact of Proposition 13—or a related tax measure—on local governments. The future of the property tax and the search for alternative local revenue sources merit fuller treatment than is given in Henry Maier's short essay.

The section on policy suffers from similar problems of restricted coverage and focus. The section does not treat the impact of retrenchment on public safety, a curious omission in light of the fact that cutbacks in the number of police and firefighters commonly signal the new austerity in cities' budget making. Recent congressional attention to welfare reform and the link between welfare costs and the fiscal troubles of cities make the neglect of welfare policy, both in separate articles and within existing contributions, seem inexplicable. By the same token, it is difficult to understand the inclusion of two or three contributions (such as Gary Gappert's "Employment Policy in Post-affluent America" and Ann Lennarson Greer's "Health Care Policy: Disillusion and Confusion") which, whatever their merits otherwise, relate only indirectly to the retrenchment theme.

The most serious shortcoming of the volume is the collective failure of the contributors to move beyond the identification of broad trends to the kind of precise analysis that could be profitably applied to the circumstances of particular cities. Such analysis could embrace quantitatively oriented comparative studies of the conditions associated with fiscal distress as well as descriptive case studies of specific cities. The contributors make little effort to isolate the types of cities likely to experience greatest hardship in the coming years or—with the partial exception of the article by Warner Bloomberg, Jr., on the growing Hispanic influence in southwestern cities—to consider the regional impact of retrenchment. Moreover, it is surprising not to see a case study of the bankruptcy crisis in New York or Cleveland in a collection of this sort.

The weaknesses of the volume diminish its value either as a supplementary textbook for courses in urban government or as a compendium of the relevant literature for scholars interested in the subject. However, the quality of some of the individual contributions make the book worthy of attention. Apart from the articles by the editors and Mikesell already noted, several others deserve special mention. Harold M. Rose provides a cogent review of black economic progress and gives critical attention to the relative influence of class and race in determining minority economic success. David H. Rosenbloom's article on city personnel policies makes the interesting point that merit selection rules and union power have significantly reduced the supply of patronage jobs and thus have undercut the development of black political machines along the lines of earlier ethnic organizations.

What is perhaps the most significant observation of the volume arises out of separate articles by Theodore J. Lowi and Scott Greer. Lowi sees the emergence of a "Second Republic for the Cities" in which they act "increasingly as agents of the federal government" (p. 50). Greer, in turn, argues that the regulations accompanying federal aid have increased the bureaucratization of city politics, a trend enhanced by the expanding ranks of public service unions. Federalization and bureaucratization have combined to minimize the flexibility of urban governments in responding to retrenchment. In short, modern cities may hold little control over their political futures.

TIMOTHY G. O'ROURKE

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The Presidential Election Game. By Steven J. Brams. (New Haven, Conn.: Yale University Press, 1978. Pp. xix + 242. $15.00, cloth; $3.95, paper.)

The Presidential Election Game is more nearly a collection of discrete essays largely devoted to aspects of electoral strategy in presidential contests than it is a book unified by either a coherently focused empirical study or a body of theory. Indeed, what unites this substantively disparate volume is what Abraham Kaplan has called "the law of the instrument." Brams' instrument is formal theory. He hammers with it incessantly, and vigorously pleads its case as a means of generating both theoretical insight and the intellectual discipline to condition those insights.

I suspect that there can be no single fair review of *The Presidential Election Game* in all its aspects because much depends upon the reader's intellectual temperament and the extent to which it accords with that of Brams. My hope is that the sum of the reviews will provide a reasonably balanced picture. I feel compelled to raise this point because undoubtedly some will be irresistibly attracted to the dulcet elegancies of Brams' formal models, and may find in them the attractiveness of disciplined insight which they are advertised as offering. For myself, I find the lure mostly resistible.

Brams' modeling works best when it illuminates the implications of alternative choices, as it does in his last chapter which deals with alternative methods of voting for application mainly in primary elections. Three voting systems are offered for examination–plurality, negative, and approval voting. They are assessed with respect to their various capacities for meeting the criterion of acceptability in multicandidate elections. Even here, however, Brams' assumptions can be large. For Brams fails to consider the substantial information requirements made of voters trying to decide how many of several candidates are acceptable. What, for example, of information constraints upon voter behavior under a system of approval voting? When voters have the freedom to find more than one candidate acceptable, are they likely to find acceptable a candidate about whom they know little? Or are they likely to not approve a candidate about whom they have little information? Nonetheless, whatever disturbances are introduced empirically by information constraints upon the operation of alternative voting models for meeting the acceptability criterion, Brams' deductive modeling is most useful here in illustrating the formal properties of alternative methods of choice. Stated slightly differently, formal or deductive treatments work best when the questions they address are fundamentally analytic rather than empirical.

But when Brams' theoretical focus lies in developing coherently deducible explanations of political behavior, his assumptions often become more questionable and his elegancies come to rest upon a reconstructed logic which in some instances cannot be tested, and in other instances flies in the face of substantial bodies of empirically derived knowledge. In his first chapter, Brams tries to demonstrate, much as Anthony Downs had earlier, that candidates will try to maneuver strategically across a one-dimensional space representing the distribution of voter preferences. The criticism leveled at Downs by Donald Stokes ("Spatial Models of Party Competition," *American Political Science Review* 57: 368 f.) for introducing simplicity of assumption at the cost of empirical validity holds even more forcefully now with the surfacing of a significant body of empirical literature dealing with the multidimensionality of candidate evaluation. It appears from Brams' own testimony that he is hardly unaware of this, but that he might find it inconvenient to consider such complicating information directly. As he puts it: "Multiple issues greatly complicate–and may ultimately confound–single issue spatial analysis. . . . Nevertheless . . . spatial analysis provides a useful framework within which to relate these characteristics [candidate positions and voter attitudes] in both single elections and a series of primaries" (p. 39). Unfortunately, the precision which Brams brings to building his models is absent when it comes to specifying their utility.

In chapter 2, Brams devotes great effort to modeling the behavior of convention delegates. In doing so, he assumes that they are rational and that there are spoils to be shared. His focus upon the convention process itself is based upon a multiple-ballot convention and is disconnected both from the primary process and, I am afraid, from reality. In essence, Brams does a wonderful job of modeling the 1924 Democratic Convention. Linking the dramatic proliferation of primaries and state caucuses to the altered state of the conventions themselves, however, is not a matter that Brams finds worthy of attention. Yet the primary process seems to be linked to the absence of multiple ballots at conventions. Increasing numbers of delegates arrive at the convention committed merely to ratifying decisions reached by broader selectorates. Additionally, decreasing numbers of them are professional politicians. What spoils are they seeking? Who has anything to give them? Surely, in 1976, Gerry Ford was searching to find takers for whatever spoils he had at his disposal without finding an overwhelming number of takers. Toward the conclusion of this particular chapter, Brams makes the claim, repeated in slightly different form throughout the book, that his models which assume goals lead to theoretically based explanations, whereas analyzing the motivations of actors leads only to bizarre or ad hoc interpretations. The problem, of course, is one of correspondence, i.e., does the reconstructed logic coincide with the logic in use? If the answer is yes, then Brams has valid theory. If no, then the presidential election game isn't the only game that is being played.

The middle chapters of Brams' book deal with the strategic allocations that candidates should make under the Electoral College system in the general election, the problem of holding coalitions together and its relationship to the size principle, and the strategies of Supreme Court Justices Burger and Blackmun and President Nixon vis-à-vis the Watergate tapes case. In the latter chapter, Chief Justice Burger and Justice Blackmun are characterized as a single actor so that a two-person game with Nixon as the opponent can be constructed. Since the game assumes their motivations, perhaps Brams should be given an opportunity to do a review of *The Brethren.*

To conclude, I am not sure how this book will be received by the "let us assume a can-opener" partisans, but among those of us who are either more unskilled or less obsessed and who have an interest in theoretically connecting presidential selection processes to presidential governance, *The Presidential Election Game* will seem to be occasionally inspired, but mostly trite. In brief, the book is longer on theoretical pretense than on theoretical payoff.

BERT A. ROCKMAN

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The Crises of Power: An Interpretation of United States Foreign Policy During the Kissinger Years. By Seyom Brown. (New York: Columbia University Press, 1979. Pp. xi + 170. $10.95.)

Since World War II, each successive American foreign policy era has been intensively scrutinized. It is somehow fitting that the era just past should be popularly named for Henry Kissinger. With the Vietnam debacle, Americans were conscious as never before of the limits of their power. For the first time since the American Revolution, they were being obliged to reorient their foreign policy toward the systematic practice of balance-of-power politics. On the accession of the Nixon administration, at the president's elbow and perhaps not entirely by chance, was a professor steeped in the study and operation of these tactics.

The Kissinger years are selectively highlighted in crises by Seyom Brown's insightful essay. Starting with the inheritance of confusion, typified by Vietnam, he turns to the Mideast, the world economic order and oil, and to Africa as they are thrust upon Kissinger, demanding decisions. Deliberately presenting his analysis before Kissinger's memoirs appeared, Brown believes it preferable to have his own measured views on record before that of the defense. The technique succeeds because of his sophistication in cutting through to the basics of Kissinger's reasoning and motivations as stemming from his *realpolitik* view of the world.

As a setting for the Kissinger era, however, Brown could have dwelt upon some preparatory factors that buttressed Kissinger before he strode on his own. Nixon, although referenced, is underplayed. After all, as vice-president he had been privy for eight years to the policy formulations of the Eisenhower administration, which combined with his extensive travel and top contacts, gave him an early great advantage over his lieutenant. It was the president who directed the systematic reevaluation of America's policies, and, profiting by his earlier experience, used Kissinger as the staff means to develop priorities and courses across the spectrum. And he wanted this done through White House control.

A brilliant academic was given the unique opportunity of running a super-seminar at the center of policy making, while, chairing all major interagency action committees, he could follow the effects of policies. Combined with this was his crucial control of access to information, ever more restricted as he gained confidence. By the time he took the stage in his own right, as in Brown's crises study, Kissinger had assimilated a wealth of policy and options perspectives that no other governmental figure—certainly not a domestically beset president and his internationally naive successor—could match.

There are Kissinger supporters and detractors. Brown gives judicious criticism, but personally concludes that he played a constructive role in adapting American policy to a more complex world on which our power had lessening impact. Reluctantly, he says, Kissinger also turned to relating the United States to new global forces.

Since Kissinger has an unprecedented control over the material available to judge his actions, it will take some time before a balanced picture emerges. However, Brown's study is an excellent and necessary preliminary.

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Presidential Selection: Theory and Development. By James W. Ceaser. (Princeton, N.J.: Princeton University Press, 1979. Pp. xiv + 371. $20.00, cloth; $4.95, paper.)

The presidential nominating process has begun to receive extended critical attention over the last few years. The profound changes in presidential nominating politics brought about by the party reforms of the last decade have reawakened interest in what is arguably the single most important act in the process of democratic governance, the selection of presidential nominees. James W. Ceaser's study is a noteworthy addition to the field. Its particular strengths lie in the development of broadly competing theories of presidential selection; the historical perspective applied to the understanding of selection processes and their evolution; and the elaboration of the ideas on presidential recruitment associated with key figures, or schools of thought, influential in shaping nominating procedures, from the Founding Fathers to the present.

In these latter respects, there is nothing available quite like the Ceaser book, and therein lies its major contribution. The book elaborates the broad democratic theories behind specific constitutional arrangements and it asks questions as to the representativeness and, more specifically, suitability of nominating arrangements for choosing presidents capable of functioning in a leadership capacity. It is this last emphasis that provides the study with one of its more unique foci. While implicit in other analyses, questions as to leadership potential are seldom directly addressed.

Presidential Selection is ambitious. The unifying theme concentrates on the evolution of presidential selection from the early days of the republic up to, and including, the current debate over reform. Ceaser makes a detailed analytic evaluation of the arguments, and assumptions, put forward by the Founders, Thomas Jefferson, Martin Van Buren, Woodrow Wilson, the populists, the Progressives and modern-day reformers. The author evaluates each school of thought in relation to five criteria: the ability of the processes in question to minimize disruptions in the pursuit of power; the manner in which selection practices affect the way in which presidential power is exercised; the extent to which selection procedures emphasize the personal characteristics needed for an able chief executive; the degree to which nominating systems promote legitimation, i.e., the assumption of power through means perceived to be orderly, fair and in line with the nation's democratic ethos; and the "proper" amount of choice and change offered within the process, a point of debate between the "consensual" and "responsible party" advocates.

It is Ceaser's contention that Wilsonian ideals continue to dominate today and account for much of the confusion in the debate over the adequacy (however defined) of the presidential selection system. The author argues that Wilson sought to minimize party control over nominations, favoring instead a "plebiscitary" nominating process, and, on the other hand, arguing for a responsible party system, more cohesive, disciplined and responsive that the coalitional parties of that (or this) day permit. Ceaser finds much the same destructive ambivalence in contemporary reformers.

It is not an argument that I completely accept. The responsible-party advocates of the 1950s and their elitist emphases have a tenuous relationship to the party reformers of the 1970s. It may well be that different political arrangements designed to accomplish specified party functions can coexist within a party system as organizationally loose as is the American. My major reservation with the book concerns the treatment of the contemporary debate over reform. In this discussion Ceaser appears a little less sensitive to the issues involved, their ramifications and the competing models (regulars' and reformers') of presidential selection than he is elsewhere. In this area, the book hews closely to traditional political science assumptions that, if anything, in themselves could profit from a critical reexamination.

It is not necessary to agree with everything that Ceaser says to recognize the worth of the book. There is much that could be discussed. To my knowledge, much of the information is new, at least in its relation to the nominating system, and the perspective is original and provocative. The study is a singular achievement contributing to current efforts to understand and reappraise presidential selection processes and their contribution to a vital democratic order.

WILLIAM CROTTY

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Politics of Arms Control: The Role and Effectiveness of the U.S. Arms Control and Disarmament Agency. By Duncan L. Clarke. (New York: Free Press, 1979. Pp. x + 277. $15.95.)

The U.S. Arms Control and Disarmament Agency (ACDA) was established in 1961 with congressional intent that it should assume a major role in national security affairs. Duncan Clarke, a scholarly expert in the field, has assembled a definitive record of the agency's efforts to carry out its mission. He recalls the agency's unfortunate original image, equated with "naiveté, softness and idealism," but which was subsequently improved as arms control increasingly became "a recognized component" of security policy. His close personal knowledge of most of the agency's directors enables him to cast each in terms of acceptance or non-acceptance of the arms control principle within the foreign affairs system. ACDA impact on policy is, Clarke demonstrates, a direct reflection of leadership which, in turn, is largely measured by the degree of access of the director to the president, a problem for all ACDA directors except Paul Warnke. Equally important is being able to stake out a substantive piece of foreign policy action. Although ACDA forged a leading role in connection with the 1968 Non-Proliferation Treaty, it had "no meaningful say in key defense programs affecting arms control, conventional arms sales, or the use of chemical weapons in Vietnam. Though heard, its voice in SALT was muted" (p. 212). ACDA's situation improved in the mid-1970s, especially following the departure from government of Henry Kissinger, of whom Clarke is critical. Congress began to take greater interest, mandating that departments concerned with new defense programs include program impact statements on arms control policy and negotiations.

Clarke treats at length operational problems of ACDA. They have included the difficulty of obtaining information, especially from the Department of Defense; the perennial problem of settling on the most workable relationship with the Department of State; definition of the role of the agency in actual arms control negotiations; perception that ACDA's research capability and product are not fully appreciated, and the problem of what kind of public information program to mount. ACDA is a tiny agency in the federal galaxy and Clarke, in one of his numerous prescriptive remarks, expresses his desire that it should remain such. Numerous military and foreign service officers detailed to the agency over the years have made significant contributions, the author states, although he believes the appointment by President Carter in 1978 of retired Lieutenant General George Seignious to succeed Warnke was a mistake.

The actual politics of arms control which this volume narrates are those observable in other federal agencies with foreign affairs-oriented missions, relatively few congressional advocates, and weak external constituencies. These are the second-echelon foreign affairs subsystems. The International Communication Agency and the Agency for International Development come to mind. Their bureaucracies also face the task of achieving and maintaining "inner circle" status within the national security hierarchy of senior advisers which Clarke identifies as the president's assistant for national security affairs, the secretaries of State, Defense and Treasury, the director of Central Intelligence, and the joint chiefs of staff. Second-echelon agency viability is directly linked to the quality and influence of its directors, much more so than in the cases of first-echelon elements whose large resources and statutory responsibilities guarantee status despite the quality of their leadership. The Department of State, for instance, was not overwhelmed during the Nixon years even though William Rogers, a weak secretary of state, was overwhelmed by Henry Kissinger. The second-echelon agencies regularly encounter, as does ACDA, problems of access to presidents, of obtaining fruitful informal hearings among other foreign affairs actors and of securing membership on formal interagency committees.

Thus it appears that the politics of arms control as set forth in this volume is really the bureaucratic politics of the small, specialized federal agency, politics which differ little from agency to agency. Only the subject matter is different. Clarke's work would be strengthened had he posed the politics of arms control within a larger skein. The fact is that arms control is a proposition of vast philosophical import which poses fundamental political questions. Arms control to what end, for instance, and measured by what standards? Would it not have been profitable to examine the worldwide political implications of the postwar revival of this essentially revolutionary idea or the status of the arms control concept in the contemporary international system in order to provide some standard by which the comparative effectiveness of ACDA might be judged? Another helpful addition would have been to make reference to the considerable contributions of peace research in devising means to control violence as reference points for ACDA goals.

Perhaps these criticisms are unfair to an author who clearly limits the volume's parameters in its subtitle. In any case, the main title would be more descriptive were the modifier *Bureaucratic* added to *Politics.*

Politics of Arms Control is a record of the uneven course of a government agency charged with a mission sharply distinguishable from those of all other national security actors. The volume should interest students of bureaucracy and those of foreign policy. Indeed, should arms control initiatives—such as a comprehensive test ban, an anti-satellite agreement, cessation of underground testing and mutual force reductions in Europe—be shelved in the wake of de facto demise of detente, the volume may well be the final account of U.S. arms control machinery as we know it.

JOHN H. ESTERLINE

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Circular Letters of Congressmen to Their Constituents 1789–1829, 3 vols. Edited by Noble Cunningham, Jr. (Chapel Hill: University of North Carolina Press, 1978. Pp. lxiii + 1634. $75.00, set.)

As early congressmen groped to discover their appropriate role behavior as delegates, many concluded that it required close communication with their constituents. In his circular letter to his district, James Stewart (N.C.) began with a typical preamble:

> I take it to be the duty of a Representative in Congress, after the close of the session, to render to his own Constituents as soon as practicable a brief account of such transactions as may be most interesting to them; together with a candid statement of some, at least of the reasons which may have influenced his own conduct in relation to the votes given on the most important questions (p. 1027).

Thus began the tradition of Clem Miller. Circular letters took various forms. Some were handwritten and intended for a small, special audience, while others were sent to newspapers for mass dissemination. More commonly, though, a limited number would be printed and mailed to selected constituents with instruction to pass them along to neighbors—hence, their designation as "circular letters." (Although probably an efficient distribution system, it was certainly less than a perfect one. Passed-over constituents tended to become irritable, and at least one congressman confessed to hiding behind the response "Damn that post office!")

John Adams wrote to Jefferson in 1813 that "I would give all that I am worth for a compleat collection of all those circular letters." Although this three-volume collection arrives 175 years too late for Adams and undoubtedly many of the letters have been lost forever, Adams would have been pleased with Noble Cunningham's product. Seldom has the need for a caring editor been so great, and happily, Cunningham responded with a masterful job. Just collecting these buried and disparate artifacts must have been an enormous task. Cunningham's lead essay provides an excellent introduction. As a dutiful archivist, he notes such items as when the signatures are handwritten rather than printed and the existence (and whereabouts) of differing versions of a letter. Using footnotes to clarify and correct names, places, and events, Cunningham wisely stays out of the text.

If one expects to discover the insightful gems of a Clem Miller or even lesser-grade gossip, he will be disappointed with the early congressional correspondence. It would be naive to expect so much. Theirs was a fundamentally different enterprise. Visiting Washington briefly to resolve a few weighty issues, members of Congress worked in a simpler environment. With exclusively constitutional actors following (to the degree possible) explicit constitutional role prescription, comparatively few complexities existed which required illumination. And with partisanship equated with faction, the fascinating business of coalition-building and otherwise politicking was deemed so illegitimate that even when a congressman engaged in such activities, he would have been ill-advised to tell the folks back home. Many of the references to partisan politics in fact come in the form of denials against accusations of partisan behavior. Matthew Lyon pleaded in 1808, "So far am I from concerting or connecting with them [Federalists], that although one of them is my brother-in-law, I never visited any of them, nor they me, except that brother-in-law, and he visits me only on special invitation" (p. 597).

Instead of politics, these letters straightforwardly report congressional business, frequently including budget figures and the text of legislation. Personal or regional preferences are occasionally expressed, but by and large these letters are interchangeable across districts. (One of the early departures from the *Congressional Quarterly*-like rote occurs when Congressman McArthur defends himself against the accusation of plagiarizing from a Virginia colleague's letter to his district.) Together these letters quickly become repetitious, and early on even the interested reader begins skipping some and skimming others in search for something fresh.

The collection spans the first 40 years of government and although this collection barely encroaches upon the transitional Jacksonian era, it does provide a time-series sufficiently lengthy to detect the beginnings of several "developmental" changes. For one, references to political parties become more numerous and less apologetic. Also, with the growth of the territories and the admission of new states, local demands upon Washington grow in number and diversity. Consider this remarkable recitation of constituent work by Joseph White (Florida Territory):

> When I arrived at the seat of Government, I found a mass of private and public business, connected with the Territory, pressing upon my attention, almost sufficient to appal the stoutest heart, and requiring an exertion of industry and talent, to which I felt myself entirely inadequate. Roads and Canals, Ports of Entry and Delivery, Surveys of the Coast, Light Houses, Post Offices, and Post Routes, were wanted. The Land Claims of the whole Territory, derived from the Spanish and British Governments, were to be finally settled, with special legislation upon those over the jurisdiction of the Commissioners of the United States. Donation and Pre-emption rights to be secured, innumerable private claims, under the treaty with Spain, and otherwise, to be adjusted and paid. Our Indian affairs to be attended to; Fortifications to be erected: The Laws of the Territory to be examined, and important alterations in our System of Government, to be made for the better protection of the rights of the people, and the more safe and commodious administration of justice (pp. 11331–32).

As the western states acquired the balance of power (e.g., the presidential election of 1824 in the House of Representatives), their claims became irresistible and one senses that the once-dominant opposition to federal involvement in local affairs was relegated during this period to guerrilla tactics. Well before natural selection produced the modern vote-hungry professionals, we find in these generally apolitical actors the extraordinary compatibility between their roles as delegates and as deliverers of particularized benefits.

Perhaps the most important change over this 40-year period is with the congressmen themselves. From occasional brief glimpses of introspection one finds fewer references to congressional service as "duty" and more references to it as engaging work. Compare the following two typical passages:

Passage 1 (dated 1796):

> My dear friend, there is nothing in this service, exclusive of the confidence and gratitude of my constituents, worth the sacrifice. The enhanced price of subsistance renders it at present unprofitable, and unworthy of the talents essential to the service; the business is arduous, when alone, on dry books, writing, and study, when in company, in debate, counteracted in our best designs, and often on the brink of success disappointed; and far removed from domestic happiness, cut off from family and friends. For this no pecuniary equivalent is adequate; nothing but a consciousness of having discharged my duty, and of having obtained full confidence, could be to me a complete reward. Having secured this, I could freely give place to any fellow citizen, that others too might obtain the consolation due to faithful service (p. 57).

Passage 2 (dated 1826):

> When I first came to Congress I was young, inexperienced, and therefore but little committee business, or business out of the House, was thrown upon me. This gave me leisure, when the House was not in session, to attend to my constituents. The experience of two sessions, however (it was, no doubt, considered), had, in a degree, qualified me for a more active part in the preparatory stages of business for the subsequent action in the House. I was therefore placed, at the commencement of the session, on the Committee of Indian Affairs. The importance of this committee, and the time necessary for disposing of the various subjects referred to it . . . embraces all the subjects, all the interests and matters, in any way relating to, or connected with, any and every Indian tribe throughout the limits of the United States. This, I hope, will be received as a sufficient apology for my seeming negligence (p. 1424).

These letters provide invaluable information on early congressmen's views on issues of the day, on Washington, and on themselves as public officials. Students of Congress are heavily indebted to Noble Cunningham for introducing us to them.

SAMUEL KERNELL

University of California, San Diego

The Process is the Punishment: Handling Cases in a Lower Criminal Court. By Malcolm M. Feeley. (New York: Russell Sage Foundation, 1979. Pp. xxii + 323. $10.00, paper.)

It seems commonplace today to use organization models for understanding dispute processing within state trial court systems. Indeed, one might even suggest that this paradigm, which focuses upon interactions and negotiations among multiple actors within the system and which accordingly minimizes the importance of the judge in fashioning the outcome, has rapidly emerged as *sine qua non* among the amalgam of scholars from sociology, anthropology, law and political science drawn towards the study of law and courts. Usually this genre's product is the case study of a local court, although some (like Eisenstein's and Jacob's *Felony Justice*) develop a narrow comparative model. Whether or not all this will have a sanguine effect upon political science perhaps remains to be seen since, constrained as these studies typically are by the individual-case approach, the ultimate success of any one effort depends heavily upon the amount of theoretical clarification gained by structuring one's design within a single unit when measured against what is sacrificed by the purposely narrowed focus. Choice of locale is obviously important to this balance; here, it apparently coincided with Feeley's association with the Yale Law School. Ultimately we should be unconcerned with whether or not New Haven is unique among or comparable to other American cities, and should be very much attuned, instead, to how Feeley's sojourn in New England enlightens us about criminal court dispositions. Moreover, those who would quickly chastise Feeley for providing only a case study of New Haven's Court of Common Pleas, the lower tier of a bifurcated criminal court system which handles lesser felonies and misdemeanors, should be alerted to the fact that the author is pursuing the medium of discovery, not proof, and that both modes of inquiry are essentially compatible with "good" social science. Thus, whether or not the balancing of research strategies is optimally beneficial is very much dependent on what we discover in the final product.

This book is organized as one might expect. The reader is presented initially with a summary of prevailing models found in the criminal justice literature. Here we sift through open and closed court systems, due process and plea bargain models, and the like. The chapter also introduces us to the central theme: that for a defendant to meander through the criminal justice process is ironically more of a punishment than is the formal sanction applied by the court. This is not necessarily a novel interpretation of the administration of justice, but an interesting one nevertheless. Before moving towards a test of the hypothesis, however, we are led through two separate but important stages of the research. The first is 83 pages describing the New Haven Court of Common Pleas and its political context. I suppose New Haven fans would be charmed by this tour, but one must question the wisdom of devoting over 25 percent of the book at this stage, and even more later, to describing a court which processes misdemeanors and lesser felonies. More likely than not, the reader might be less troubled here had the anticipated logical nexus actually appeared between what we learn about a fundamentally uninteresting court and the hypothesis advanced in the opening chapter. Unfortunately it does not. This description does, however, provide the setting for what is typically the intent of many trial court system studies and for what seems to be Feeley's original purpose in conducting this research, namely, testing the impact of a host of legal, social and structural variables upon case outcomes. In this second stage, Feeley finds weak relationships between 27 indicators on the one hand, and adjudication outcomes (nolle or guilty) and sentence severity on the other.

Here we reach a critical juncture inasmuch as Feeley freely admits to the statistical model's inadequacy. But rather than torturing the data to make it explain more variance, he instead chucks the entire model, wraps himself in the mantle of participant observer/ethnographer, and sets out to discover the variables that do explain court outcomes yet which had eluded him in the statistical analysis. It would be foolhardy to disagree with Feeley's conclusion that the statistical model was inadequate since the battery of indicators explain slightly more than one-third of the variance in adjudication outcomes and only 12 percent on the sentence severity measure. But one might be struck by the ease in which he dismisses the statistical model almost to the extent of suspecting that he had never intended it to work in the first place. And before discarding it, one might pause to consider some explanations for its failure. For example, it may be that the variables entered into the regression equation are "correct" in the sense that they are specified as relevant by prevailing theories of trial court decision making. If true, the failure to explain outcomes may stem from how these variables are measured; and much of what we know about the accuracy of courthouse records, how they are collected and how they are aggregated, suggests that one should be eternally wary about their reliability. Alternatively, the variable measurement is satisfactory, but the theory which specifies which variables are to be included in the analysis is faulty. Obviously, it is difficult to decipher which of these possibilities (or others) are to blame, nor have we not yet developed a divining rod to easily point the correct direction. Thus Feeley may be on the mark by wanting to immerse himself in his research site in order to ferret out the relevant variables like a hunter with so many hounds. Unfortunately, a hunter would usually have an advantage on Feeley in that the former at least knows what his quarry looks like, and presumably so do the hounds. Feeley, unfortunately is in the more precarious position of not knowing precisely what he is looking for.

I emphasize this quandary because the next several chapters more directly address Feeley's primary contention: that formal sanctions imposed by the Court of Common Pleas are minimal; that the psychological, social and economic costs incurred by defendants flowing from pretrial detention, securing legal assistance, and from continuances which delay final resolution, all of which occur prior to final disposition of a case, are harsher *de facto* penalties than are the sanctions actually imposed by the court. This plausible hypothesis, which weaves through the remainder of the book like a *basso continuo* motif, may indeed explain the failure of the statistical model to provide a satisfactory explanation for case outcomes. Curiously, however, no direct test of it is ever presented. Instead, other themes emerge and disappear like so many digressions to the extent that Feeley never confronts his hypothesis head-on. At times (pp. 145–88), we are provided a further trek through this particular New Haven court. Arguably some of that is inevitably bound up with describing informal case dispositions but this does not by itself substantiate the central hypotheses. Likewise, in chapter 8 ("Myth of Heavy Caseloads") Feeley argues that informal case disposition and heavy caseloads are not necessarily correlated. Again, this would lend credence to his proposition, but by itself does not constitute a test of it. In part, the problem seems to stem from the quality of the data available compounded by the inferences Feeley wishes to draw from them. For example, an "imbalance ratio" is devised to describe the proportion of defendants who incur pretrial detention to those who receive post-conviction incarceration. The author tells us (p. 222) that "roughly four times as many people were incarcerated before disposition than *after* disposition. . . ." But if we take the author's data at face value, Table 7.2 also informs us that 94 percent of defendant-universe had been held less than 24 hours and 60 percent detained three hours or less. Thus there is not much variance in this cost measure. Of course, there are other indicators of "cost" such as the continuances and those incurred by obtaining legal assistance. Yet again, these arguments simply support the plausibility of the "process-is-the-punishment" hypothesis and should not be confused with a test of it.

Feeley seems to have neglected what may well have been the most critical stage of the research. If psychological costs, and not formal sanctions, are the deterrent meted out by lower criminal courts, then the defendants' perceptions of these impediments affecting economic security, employment, or social relations should be the next stage of inquiry. This task was obviously apparent to some degree because Feeley at one point writes: "While I did not systematically interview a sample of defendants, I had informal and often extended discussions with dozens of defendants . . . and I watched still more discuss their cases with attorney's and prosecutors. While there were obvious and numerous exceptions, I was nevertheless struck by the frequent lack of concern about the stigma of conviction. . ." (p. 190). If, as the author then concludes, the "psychological costs of the criminal sanction are diminished" such that "the process, itself becomes the punishment" (p. 191), we anticipate a more systematic test of this hypothesis. It never appears. Here we must come to grips with the framework which Feeley has adopted. Ethnography as borrowed from cultural anthropology undoubtedly is useful in the realm of discovery. However, it's inadequate for ethologists/political scientists to immerse themselves in a local court system and expect the data necessary to test a hypothesis to soak in like so much empirical marinade. Insights will obviously emerge by proximity to one's research site but this enlightenment is not tantamount to proof of suspected linkages among variables. To do no more than assimilate the spirit of trial court decision making without partaking of the social science skills that I know Feeley possesses does not do justice to our understanding of justice.

BURTON M. ATKINS

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Foreign Policy By Congress. By Thomas M. Franck and Edward Weisband. (New York: Oxford University Press, 1979. Pp. ix + 357. $15.95.)

Those who have been trumpeting great changes in Congress' foreign policy role during the seventies, but fumbling for details after cursory mention of the War Powers Act, will find Thomas M. Franck's and Edward Weisband's *Foreign Policy By Congress* a godsend. Franck and Weisband offer a detailed, ambitious, and much-needed examination of evolution in the post-Vietnam foreign policy role of Congress.

The authors' major argument is that a lasting revolution in Congress' foreign policy role occurred in the mid-seventies—a revolution not to be confused with a mere swing of the proverbial pendulum of institutional power. The permanence of this transformation is evidenced by legislated procedural changes, an independent congressional policy capability, institutional democratization, the politicization of foreign policy issues, structural accommodations by the executive, and a favorable cost/benefit ratio in recent congressional foreign policy activism. In summary form, the authors characterize this revolution as "the defeat of politics by law" (p. 155).

The evidence for a permanent, positive, revolutionary change is divided in three distinctive parts. First, the authors present three case studies of legislative contravention of executive policies: a legislated peace in Indochina, an arms embargo against Turkey, and the termination of assistance to pro-Western factions in Angola. The three examples are skillfully used to highlight important themes: the rising importance of the rank and file in Congress, the lasting importance (and occasional predominance) of institutional competition relative to substantive concerns, and the ever-present impact of domestic politics on foreign policy making.

Part 2, clearly the heart of the argument, examines the extensive procedural changes wrought by a recalcitrant Congress—in war powers, human rights, military aid, nuclear exports, intelligence, and treaties and executive agreements. Several organizational problems weaken these chapters. The provisions of the War Powers Act are not well summarized. This makes the evaluation of impact difficult for the reader. Important differences between the House and Senate Select Intelligence Committees are not effectively summarized either, with similar results. And, interestingly, there is little discussion here of foreign economic policy despite the authors' admission of its importance. On balance, however, the case made for significant procedural change is well documented, valuable, and (it seems to me) quite correct. But the argument that the change is positive and permanent is not so easily made. Opponents of legislative meddling in foreign policy will not be persuaded by the evidence presented here; they will choose instead to reinterpret these cases or marshal their own examples of legislative blundering. Whether or not the changes are permanent remains to be seen. It would seem sufficient to simply recognize their importance for the near future.

Part 3, the weakest link in the book, is largely a description of structural evolution in the institution—with emphasis on interest groups, floor procedure, the party leadership, the party caucuses, committees, and staff support. In particular, legislative scholars are likely to be disappointed in these chapters. Relatively little time is spent analyzing structural, staff, and personnel changes in the two foreign policy committees—though one picks up details elsewhere in the book. The book presents no systematic evidence for change (or lack of change) in the ideological makeup of the committees. And the discussion of some topics —appropriations, committee jurisdiction, the budget process, and the role of party leadership, for example—is either too superficial or not effectively organized. Thus, legislative scholars will not find much that is new here, although several potentially intriguing hypotheses are suggested: examples are the effects of staff, the new oversight, and floor amendments on legislative participation in foreign policy, or the ideological relationships of members and staff.

Weisband and Franck have written a valuable book in an area too long ignored by foreign policy analysts and legislative scholars. Readers of *Foreign Policy By Congress,* of which there should be many, are in for a rich but occasionally frustrating experience. The book is rich in detail, in potential for future research, and in ammunition for supporting a case for legislative contributions to foreign policy making, but the argument is not always well organized, supported, or summarized.

CHRISTOPHER J. DEERING

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Catholics and American Politics. By Mary T. Hanna. (Cambridge, Mass.: Harvard University Press, 1979. Pp. ix + 262. $15.00.)

It was a pleasure to read an account of Catholics and the Catholic church in American politics that is an honest attempt to get at reality. Reality, as perceived and reported by Mary Hanna, is many-faceted and complex, and provides ammunition rather even-handedly to defenders and opponents of the Catholic church in America. More importantly, the book provides a new scope and depth of understanding of the reactions of Catholics and their church to political parties and political events and political candidates.

The book's scope includes descriptions, data, and analyses, of the leadership of the Catholic church in America, Catholics in Congress, and rank-and-file Catholics. Depth is provided by a detailed study of the abortion controversy in particular and how this has inspired the Church to wage war with secular America along the lines of life versus the destruction of life, a battle which has given the church new unity and inspiration.

Data concerning the leadership of the Church were obtained through open-ended "elite" interviewing. Hanna concludes from her data that leadership has changed in at least two ways. First, power is more widely diffused and not hierarchically assigned and employed. Much power is enjoyed by the "professionals" or church bureaucrats, who are also theological and political liberals. The bishops do not govern. Bishops are "kind, gentle, amiable, . . . men of good appearance, kind, ceremonial figures who will give stability. . . ." The ideas do not come from the bishops. Influence and power lie with the professionals who aid them. Power and influence also reside in activist priests and lay people.

In Congress, Hanna finds that Catholics are increasingly in evidence, both in terms of numbers and influence. In numbers, the proportion of Catholics in Congress approximately equals their proportion in the general population, a bit above 20 percent. In terms of influence, Catholics in the United States Senate include Edward Kennedy and Ed Muskie, and in the House include Thomas "Tip" O'Neill; and Catholics chair about one-third of the standing committees in the House of Representatives. However, Hanna concludes that the influence of the Church as an institution has declined even as its numbers have grown in Congress. Interestingly, older members of Congress view this decline benignly as a desirable concomitant of assimilation. Younger Catholic congressmen, however, view the decline angrily and blame it on insufficiently developed political consciousness and skills on the part of the Church. It seems that younger Catholic congressmen tend to be political liberals and want the Church to stand with them on social and political issues.

The reason for the decline of the Church's influence in Congress is that there is no longer just one way to be Catholic. According to Hanna, members of Congress "seemed to have chosen from among several equally genuine, equally valid Catholic traditions and built their lives and thinking around them." There is no single thrust that Catholic religious influence takes in the lives and thinking of the Catholic sector of Congress. Catholics in Congress include Christian Democrats, ethnic Catholics, Council Catholics, Jansenists, and "transitional" Catholics, who accept some Council reforms and are dubious of others.

The third Catholic group discussed was rank-and-file members of the Catholic church. The data were taken from surveys of the National Opinion Research Center of the adult population of the United States. Catholics occupy a "middle" position in American society. Catholics are "middle" in place of residence, in that most live in the suburbs and small cities. Few Catholics are to be found "down on the farm." Catholics are also "middle" socially and economically, in that the average income of white Catholics is only exceeded by that of Jews. However, Catholics do not fare as well in terms of percentages occupying high-prestige positions. Catholics are also in a middle position on social and political issues. On civil liberties and economic issues, Catholics are more liberal than Protestants and less liberal than Jews. On issues concerning minority rights, such as busing, Catholics occupy a more conservative position than Protestants. However, more striking than the differences is the broad similarity in attitudes of Catholics and Protestants.

One issue which Hanna analyzes in depth and at length, is the position of Catholics and the Catholic church on abortion. She finds that the leadership of the Church, Catholics in Congress, and rank-and-file Catholics depart from other groups in the population on this issue. Further, she concludes that abortion is simply the tip of an iceberg of issues, each spinning off from the body of issues. The body of issues is the right to life and qualitatively defines the Catholic position and distinguishes it from that of secular America. Most Catholics are happy with drawing a line and drawing it along this constellation of issues, for they feel that it is a line along which they must fight for their survival and the survival of the society.

JOHN FENTON

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Dimensions in Urban History: Historical and Social Science Perspectives on Middle-Size American Cities. By J. Rogers Hollingsworth and Ellen Jane Hollingsworth. (Madison: University of Wisconsin Press, 1979. Pp. viii + 184. $19.50.)

This is an ambitious book—and a strange one. J. Rogers and Ellen Jane Hollingsworth describe the volume as "several essays which focus on how social and economic factors have impinged on politics in American cities since 1870" (p. 3). Relationships among social, economic and political factors in American urban development are discussed in 174 pages of the text, but the book is also preoccupied with the historical classification of cities (two of four chapters are devoted to this) and with other matters as well.

There is a great deal to chew on here, yet the authors' description of the book is overstated. A vast interdisciplinary literature is assayed and a truly laudable attempt is made to integrate the essays with previous research in urban studies; 179 footnotes cite almost 400 different sources. What emerges, however, is highly incomplete. The book would have been more appropriately titled, "Working Papers in the History of American Urban Politics."

In the opening chapter, "Types of American Cities," the authors construct "ideal types" to specify "the dimensions by which the political world varies from one city to another, as well as the social and economic dimensions which shape the political characteristics of cities" (p. 8). Five types of cities, "autocratic," "oligarchic," "polyarchic," "elite suburb" and "upper middle class suburb" are created. The ideal types are said to be based on "most American cities smaller than 250,000 during the period since 1870" (p. 8). However, we are never told what "most" means, nor are we given a justification for the focus, in much of the book, on cities of "middle size." An assertion is made (p. 5) that "very large cities have a political process different from that of the smaller city [how much smaller? where is the size threshold?] because of their higher population density [why "density"?] and because of the unique functions which they play in a hierarchy of cities [which unique functions? what sort of hierarchy?]. The statement raises many more questions than it answers.

The five city types have a familiar ring. They apparently have their roots in the standard literature on urban politics and society. There is an intriguing attempt to relate the five types to Roland J. Liebert's important work on city government functions but, on the whole, the types do not offer much that is new.

In an abrupt shift, chapter 2 consists of case studies of Eau Claire, Janesville, and Green Bay, Wisconsin, circa 1870–1900. The cases, drawing on a wealth of material, much of it unpublished, are well done and intrinsically interesting, but they are less relevant to the preceding typological analysis than the authors contend. The three communities were lumbering towns ("oligarchic" types) in the 1870s, and as their economies changed with the depletion of surrounding forests, political and social changes occurred as well. However, the links between these changes are not well established. It is argued, for instance, that, "while there was more conflict in Eau Claire than in Green Bay or Janesville during the late nineteenth century, the ethnocultural diversity in Eau Claire was sufficient to keep class conflict at a minimum" (p. 66). But the assertion about Eau Claire is not disconfirmable since, in effect, however much class conflict there was, it is said to be the minimum possible because of ethnocultural diversity.

Chapter 3 is titled, "The Impact of Voting Behavior on Public Policy." The authors construct an "input-output" model relating data on local expenditures and electoral participation for 154 cities for the 1870–1900 period. As with similar studies of contemporary cities, the political variables tend to "wash out." While the analysis is an interesting extension of the "input-output" model to historical data, two criticisms may be made. First, much of the statistical analysis presented is incompletely detailed, making interpretation difficult for the reader. More troubling is the clash of multivariate analysis with pure conjecture. This is exemplified by the statement (p. 139) that "people probably participated at higher levels in local politics [than in state elections] because they understood local issues better than those elsewhere and because they perceived that local decisions had more effect on their lives than those made at higher levels." An equally plausible hypothesis for the differences in local and state turnout in the 1870–1900 period–that urban political machines were better able to mobilize voters in local contests–is not considered.

A final chapter, only 15 pages in length, once again returns to city classification. The authors are quite properly critical of the factor analysis approach to city classification and substitute another input-output model; 278 American cities in the 10,000–25,000 size range "during the late 19th and early 20th century" (p. 164) are arrayed in a densely packed 3 x 3 matrix. Once again, the results are interesting but inadequately analyzed.

HARVEY BOULAY

Boston University

The Right of Mobility. By Gerald L. Houseman. (Port Washington, N.Y.: Kennikat Press, 1979. Pp. x + 111. $12.50.)

This is a specific examination of the rights of mobility and of the various critiques of those rights by an author whose general research interests center upon human rights questions in urban and industrial environments. It deals in depth with the decision-making structures and processes which explain the general lack of recognition given to rights of mobility and the disadvantaged position these rights occupy, the objective possibilities of legal and constitutional enforcement, and the alternatives of statutory remedy and constitutional amendment.

As C. Herman Pritchett points out in the foreword, and as most legal scholars know, rights of mobility, like rights of privacy, are not expressly mentioned in the United States Constitution even though they are recognized as "basic" if not "essential" elements of the human condition in nearly every post-feudal society. In American constitutional law, therefore, when the Supreme Court has had to deal with them (as regards marriage [privacy] and travel [mobility]) it has had to appeal to sources that are somehow beyond the Constitution, i.e., to appeal to extra-constitutional sources ("abscribe the source of the right") or to supra-constitutional sources (Justice Douglas' "penumbras"). Thus, though the rights of mobility are highly prized, they are difficult to study systematically.

Gerald Houseman's approach to the problem is essentially that of the sociological jurisprudent. It is a legal approach but not that of a legalist. That is to say, Houseman recognizes as he should that a civil right is more than a legal concept. Law, alone, does not define a right. Rather, there is a moral basis of such rights, and here of the right of mobility. Hence, his approach is both *ideological* (defined as an action-related set of ideas) because he is concerned with the process of transferring a moral concept into law and public policy, and *teleological* because he attempts to provide a standard by which to measure the validity of law and public policy. He recognizes at the outset that law, more often than not, is a conservatizing force which prevents the expression and the expansion of such rights as those of mobility.

His concept of mobility, of course, goes far beyond that of mere "travel" to embrace "the right to passport ownership, welfare rights regardless of state of residence, transportation policy, immigration and emigration, job mobility . . . [and] social mobility" (p. 10). Judging the content of this brave book, I would suggest that a better title might read, "the paradoxes of mobility," because what Houseman deals with in fact is *mobility as a public policy issue fraught with ambiguous and contradictory value preferences.* While the automobile may be the principal contemporary agent of mobility, it produces great concern for other important values (the destruction of public transportation, the environment, and natural resources). While some of the old limitations on mobility are being eradicated (e.g., policies now benefiting the handicapped), other new limitations are being erected (e.g., no-growth movements challenging traditional freedoms of movement and exclusionary zoning laws containing privileged suburbs). While new highways have temporarily eased transportation problems, they have also disrupted mobility patterns, neighborhoods, and lifestyles. While some people have been advantaged by inexpensive FHA mortgages for low-density suburban housing, this same development has produced a congestion detrimental to everyone's mobility. a cost in energy resources, and a complex disservice to the inner city.

Houseman's prescriptions include an amendment to the Constitution (though he admits this is a tricky business and his chapter on this matter is a highly sophisticated treatment of the considerations and strategies, both favorable to and lodged against the amendment approach) and the creation of a public or private agency staffed and equipped to monitor progress and setbacks in the mobility policy field. In the meantime, Houseman is a persuasive and piquing provocateur.

William C. Louthan

Ohio Wesleyan University

The Private World of Congress. By Rochelle Jones and Peter Woll. (New York: Free Press, 1979. Pp. vii + 264. $14.95.)

This book, a first-rate theoretical essay, means to be very provocative. Rochelle Jones, an experienced Senate aide, former congressional fellow and Washington journalist, and political scientist Peter Woll seek to refute David R. Mayhew's *Congress: The Electoral Connection* (1974). They insist that "overall the goal of achieving power and status in Congress offers the single best explanation for the individual behavior of members of Congress and the collective behavior of Congress itself" (p. 10). Lest there be any doubt about their intent, where Mayhew holds that "if a group of planners sat down and tried to design a pair of American national assemblies with the goal of serving members' electoral needs year in and year out, they would be hard pressed to improve on what exist" (p. 82), they state that the "members have created an internal structure that is remarkably suitable for the pursuit of their goals of power and status" (p. 29).

The inducement of power within the Congress therefore impels members to introduce important bills even though they may be of marginal interest to their constituents. Congressman Paul Rogers is described as having achieved his reputation for clout by developing and passing health legislation. Senator Ted Kennedy sought to expand the jurisdiction of his Refugees and Escapees Subcommittee and his Administrative Practice and Procedure Subcommittee to establish himself as a Senate force.

Struggles over energy, food stamps, oversight of intelligence activities and tax legislation are among the battles described in rich, dramatic detail, each from the perspective of House members and senators seeking to defend their jurisdictional turf of to enlarge it. Senator Sam Nunn is an illustration of how a shrewd freshman can command influence by skillful use of the Manpower and Personnel Subcommittee of the Armed Services Committee. And, of course, the legendary Wayne Hays was the quintessential member who could transform a minor committee–House Administration–into a stronghold of punishments and rewards.

So with staff. They represent power and status, and the more of them one has, the greater the influence. Jones and Woll not only trace the rise in their numbers but also the increased dependence on their staffs by harassed members of Congress. The extraordinary careers of Laurence Woodworth of the Joint Committee on Internal Revenue Taxation and Harley Dirks of the Appropriations Subcommittee of Labor and HEW are fulsomely described. The authors note that the top Senate staffers belong to the Chairmen's Representatives, an association with enough prestige to attract Frank Moore's respect.

Whether it is an office with a view of the Capitol or a particularly choice parking space, perks are prized primarily because their unequal distribution proclaims to their colleagues who has the power. The recital of the tug-of-war over which members would accompany the Magna Carta on its trip from England to the U.S. Capitol is sitcom at its best.

In their excellent chapter on congressional reform, the authors describe the fate of the 1974 Bolling Committee's recommendations in the House and the 1977 Stevenson Committee's proposals for the Senate, noting how the rhetoric of reform was so often a mask for internal power struggles in each chamber.

Notwithstanding their description of the Congress as an "acquisitive society," the authors end on an upbeat note, insisting that the existence of multiple centers of power guarantees that legislation will only result from a consensus and that the world of the Congress they portray is indeed compatible with the demands of democracy. But their advice to any president is clear: don't appeal to the people; give your proposals to powerful chairmen who are searching for fresh ideas to enhance their authority.

Despite the cogency and skills of their argument, somehow the major thesis is inconclusive. Perhaps Congressman Rogers saw no electoral connection in his interest in health legislation, but the fact is that he represented a district with one of the highest percentages of older voters in the nation. The suggestion that Senator Kennedy set aside his presidential ambition in 1971 (p. 60) and proceeded to pursue traditional Senate goals is, to say the least, belied by recent events. To suggest further that even the members from safe seats battle for legislation, staff and status because they are concerned with how they are perceived by their colleagues rather than their constituents contradicts other findings. For example, Richard Fenno found in his travels that even the members from safe districts are rarely sanguine about the next election (*Home Style*, 1978, p. 14). Safe seats aren't always that safe; Mayhew reminded us (p. 35) that from 1964 to 1972 ten House committee chairmen lost their primaries. In 1976, nine incumbent senators were beaten.

Perhaps the most difficult part of this thesis is that it requires probing the psyches of members of Congress. Certainly they want to be reelected and assuredly they enjoy receiving the esteem of their colleagues, but their specific actions can serve both goals. Thus, Jones and Woll conclude that when John Brademas gave up the chair of the Subcommittee on Education to chair the Accounts Subcommittee of the Administration Committee, he was trading policy clout for inside prestige. But John Brademas has a relatively tough district, and the value of campaigning as a Majority Leader or even Speaker has certainly not escaped his attention.

If this volume tends to minimize the external pressures on the members, it is nonetheless a valuable "insider's" view. Any teacher of a course on the Congress who asks students to meet its arguments alongside those of Mayhew will generate several lively classes.

SIDNEY WISE

Franklin and Marshall College

Repealing National Prohibition. By David E. Kyvig. (Chicago: University of Chicago Press, 1980. Pp. xix + 274. $21.00.)

The era of Prohibition in America (roughly 1920–1933) is commonly stereotyped as a period of laxness in both law enforcement and public morality. Of that period, one usually conjures such mental images of numerous speakeasys, bathtub gin, racketeering and imbibing flappers—all of whom were certainly a part of the Prohibition period, to be sure. However, these were only the outward manifestations of that period, manifestations which, even today, have not been fully and completely documented.

David Kyvig's *Repealing National Prohibition* represents a serious, if somewhat limited, examination of the Prohibition period but with greater emphasis upon those forces seeking to overturn the Eighteenth Amendment (more commonly called the "dry" amendment). The book, to its credit, does include numerous new sources of materials relative to both the personalities and organizations working for repeal. Kyvig identifies the Association Against the Prohibition Amendment (AAPA) as the most important and influential of all the organizations that emerged to fight for repeal. He attributes a good deal of the AAPA's eventual success to the prestige of its membership (Jacob Raskob, Pierre and Irene Du Pont, Elihu Root, and Henry W. Westinghouse, for example), as well as to the economic resources these and others brought. The AAPA established both state and local units so as to influence local opinion in the direction of repeal. The association's basic argument for repeal was quite simple: it impinged upon states' rights. The states, at least in its view, had the constitutional authority with respect to sale and consumption of alcoholic beverages. The Eighteenth Amendment, therefore, was a constitutional intrusion of national power into matters of state power. This argument was increasingly successful as time went by in swaying both public opinion with respect to repeal as well as the opinions of members of Congress.

Kyvig also examines a number of other factors that eroded the support of Prohibitionists. He cites such factors as the increasing inability of the national government to enforce prohibition (even using the Volstead Act), the continuing success of bootleggers and racketeers in bribing state and local enforcement officials, plus the ever-increasing consumption of alcohol in the early 1920s. His thesis is, then, that a combination of political as well as social forces brought about the repeal of prohibition.

HARRY R. MAHOOD

Memphis State University

Fair Game? Inequality and Affirmative Action. By John C. Livingston. (San Francisco: W.H. Freeman and Co., 1979. Pp. xvi + 281. $12.95.)

Of the spate of publications on affirmative action since the Supreme Court's *Bakke* decision, John Livingston's book is among the most intellectually stimulating. The book is a comprehensive, scholarly defense of affirmative action reflecting the author's personal commitment to egalitarian democracy. All readers will not agree with his assumptions or his argument, but the quality of his presentation merits respect.

The author's defense of affirmative action rests on the assumption that equality is the central tenet of the American political experience. With a revivalistic fervor, he argues that equality is the source of democracy, individualism, justice, and moral politics. With the strengthening of equality, he is certain a better society will emerge.

Because he finds racism preventing equality, Livingston argues it is necessary to engage in comprehensive affirmative action programs that include racial quotas (which he refuses to distinguish from goals, as the courts have) to promote racism's eventual demise. To support his position, he quite skillfully argues that affirmative action does not create reverse discrimination or white paternalism. At length he contends that affirmative action is necessary to alleviate anti-egalitarian workings of current "merit" selection systems. Because he finds the merit ideal has roots in Social-Darwinist, capitalist, and pluralist theories, he argues that merit selection lacks the justice and compassion that should characterize American democracy.

Livingston's book invites criticism because of its passionate style. Yet the more troublesome aspects of the book are neither his logic nor political values. Most troubling is his treatment of American political thought as a conflict between the egalitarians who created American political institutions and the meritocrats and elitists who have debased the egalitarian promise. His characterization of American political thought as a conflict lacks thoroughness in exploring the varieties of American liberalism and in considering the idea of virtue in the liberal state, ignores similarities in the thinkers he contrasts, and is very limited in its review of political history. Consequently, support for his position is sometimes unconvincing.

The author also quickly excludes from consideration an elimination of the roots of racism (p. 62) or changes in an economic system that creates a scarcity of adequate opportunities and breeds racial disparities (p. 232). He is committed to the belief that affirmative action alone can alleviate the plight of blacks. Yet he presents little evidence that racial values can be altered. He ignores the literature reporting that legal changes, like school desegregation, produce minimal changes in racial attitudes. Finally, he disregards the application of affirmative action to women and other minorities.

Despite these problems, Livingston's argument retains a solidity and reasonableness that causes the reader to think about how affirmative action squares with American political ideals. Unlike so many who have recently written on politics, he forces the reader to consider the meaning of American political values in policy debates. For this reason his book merits attention.

RICHARD A. BRISBIN, JR.

Saint Mary's College,
Notre Dame, Indiana

Civil Rights and the Crisis of Liberalism: The Democratic Party 1945–1976. By John Frederick Martin. (Boulder, Colo.: Westview Press, 1979. Pp. xv + 301. $20.00.)

This book is an important contribution to the literature on liberalism and civil rights in the United States. It focuses on the growing tension within the Democratic party from 1945 to 1976 as the civil rights movement gathered momentum and then the consequent reaction set in. It is a fascinating and illuminating work.

In the early part of his book John Martin traces the strands of liberalism through the years of American history, giving particular attention to the Jacksonian era, the Civil War and Reconstruction, the conflicts between the bosses and the progressives, and the New Deal. The central concern of liberalism, dating back to the American Revolution, had been the control of power and the protection of the individual and private property. But along with the fear of power there existed a pervasive racial prejudice. The fear of power was partly shattered by Franklin Roosevelt and the New Deal under the exigencies of the Great Depression. Then, in the post-World War II years, the ideology of liberalism shifted from its preoccupation with economic concerns and came to be centered increasingly on the use of federal power in the attainment of civil rights.

Students of political parties will find the Martin volume of great interest in its treatment of the impact of the divisive civil rights issue on the Democratic party. The problem of accommodating the sharply opposing views of conservative Southerners and the Northern liberals became increasingly difficult. FDR's pragmatic approach to racial matters, as he sought to avoid antagonizing powerful Southern Democrats, had kept the factions of the party together in an uneasy coalition. It was Harry Truman's resolve to meet the civil rights issue head-on that became such a crucial turning point for the party and the nation. Civil rights became the moral issue that gave liberalism its vitality and meaning.

There is a vivid treatment of the Democratic National Convention of 1948 and the bitter split over the civil rights plank in the party platform. This ideological struggle continued to smolder in party councils throughout the 1950s—a controversy which was intensified by the decision of the Supreme Court in *Brown* v. *Board of Education.* The liberal wing of the party clearly emerged triumphant with the nomination and election of John Kennedy in 1960.

The liberal drive for civil rights culminated in the truly formidable legislative achievements of the Johnson presidency. But with the growth of the black power movement, extensive use of busing and affirmative action, a backlash of great intensity developed. The liberals came under attack from both sides—from both the conservatives and the militant radicals. Martin presents a cogent analysis of how the use of power by the liberals resulted in the contempt and resentment of many segments of American society, especially white ethnic groups, and aroused anew the old fears of government power. He is particularly incisive in his comment on certain internal actions within the Democratic party, such as the use of minority quotas for their national convention and demands in the platform for resort to more coercion, which seriously fragmented the party. Perhaps the ultimate flight from realism was reached in the unseating of Mayor Richard Daley at the 1972 convention. The old coalition of labor, liberals, and urban machines crumbled.

As the 1960s ended, feelings of failure, frustration and self-doubt beset the liberals. The mounting hostility toward the uses of federal power on questions of race now threatened to turn their triumph into defeat. The old fears of government power which the civil rights movement revived were, of course, intensified by Vietnam and Watergate. The experience of these three decades constituted a critical ordeal for liberalism within the Democratic party and the nation at large. Martin's analysis of that ordeal has been done with accuracy and insight.

RAYMOND H. GUSTESON

Ohio University

Change in Public Bureaucracies. By Marshall W. Meyer. (New York: Cambridge University Press, 1979. Pp. ix + 251. $19.95.)

The general theoretical concern of this study is the impact of environment on the structural nature of public sector organizations. It particularly considers the usefulness of the Weberian closed-system model, which suggests organizations are predominantly self-contained units, and the open-systems models, which portray extreme responsiveness to environmental pressures. A second general theoretical question concerns the likelihood of simple general theories of organizations providing great explanatory power.

The most distinctive feature of the study is the use of longitudinal analysis rather than the case studies and cross-sectional approaches which constitute virtually all the empirical work in the field. Meyer's longitudinal data set consists of a series of environmental and structural variables for 229 American state and local finance departments measured in 1966 and 1972. The project started with the University of Chicago Comparative Organization Research Program's investigation of a wide range of state and local agencies in 1966. The 1972 follow-up provides a second data point for most of the state, county (greater than 100,000) and city (greater than 50,000) finance departments in the nation.

The data analysis is presented in four chapters organized around themes relevant to major organizational change. The first concerns the structural stability of organizations. Meyer finds that increases in size over the six-year period are relatively substantial, and highly predictable as the autocorrelation between the 1966 and 1972 measures is about .95. There is also a clear, relatively predictable trend towards increased formalization of personnel procedures, and somewhat predictable trends towards decreased responsibilities for a number of finance functions and decreased competition with other agencies for finance functions. However, the autocorrelations between 1966 and 1972 levels of a series of structural variables (number of divisions, levels of supervision, number of sections within divisions, span of control) are quite low, implying low stability in organizational structures over time. This, in turn, suggests that simple closed-model theories of stability are not likely to be very useful.

The second data chapter, which concerns leadership, contains perhaps the most interesting empirical findings. The autocorrelations of structural variables over the six years are much higher within the finance departments which did not experience leadership turnover. Furthermore, the relationships between environmental and structural variables are lower among the low-turnover organizations than among their high-turnover counterparts. Meyer interprets this as suggesting that leadership can significantly limit the impact of environmental pressures upon organizations, or turn a generally open-systems description into a semi-closed system. Some additional evidence in the same vein concerns the impact of domain. Meyer finds that the domain or "turf" claimed in the formal name of the finance agency affects the relationship between environment and change in an organization. "In a sentence, comptrollers' offices are essentially stable over time, finance departments are being contracted by their environment, whereas departments of administration are growing, adding functions, and to some extent, shaping their environment" (p. 150).

In the final chapter, Meyer discusses implications for organization theory. Bureaus which operate in a world without a market mechanism appear quite vulnerable to environmental pressures, unless protected by leadership stability or modest claims to domain. Consequently, public organizations may be particularly prone to move towards hierarchy, conformity, and adherence to rules, as a substitute for the protection which can be provided by market efficiency criteria.

Obviously, Meyer (as he realizes) is well beyond his data with these speculations. Finance departments are extremely specialized, rule-bound agencies and the period studied represented a relatively turbulent time in the American political environment. Furthermore, there is no explicit comparison with private-sector firms in this study, although the popular and academic literature contain enough examples of proliferating hierarchy, conformity, and lack of innovation in both sectors to question a clear public-private distinction. However, Meyer's emphasis on time as a variable in organizational change is unique and critical. The book is a significant addition to the literature.

JOHN L. FOSTER

Southern Illinois University, Carbondale

The United States, The United Nations, and Human Rights. By A. Glenn Mower, Jr. (Westport, Conn.: Greenwood Press, 1979. Pp. xii + 215. $17.50.)

What we have here are two very slender, mostly unconnected, pieces put together to make one slender book with a misleadingly grand title. The first piece is a kind of intellectual/political biography of Eleanor Roosevelt, focusing on her activities as "the outstanding human rights leader of her generation" (p. 21). Her emergence as a great humanitarian is attributed to psychological roots, the values she learned at home and at school, and the manipulations of Louis Howe who sought ways to use her to further FDR's political career. Her influence as a human rights activist stemmed from her newspaper columning, her powerful role as in-house voice of conscience for FDR, and especially her work after FDR's death as United States representative to and chairman of the UN's Commission on Human Rights.

As a State Department official, Eleanor Roosevelt evidently was a fine team player. She listened well, gracefully accepted her duties as a government agent, and stuck pretty closely to the official line, even when the human rights cause was misused as a cold war political weapon. But Mower suggests that she was also instrumental in the development of the Universal Declaration of Human Rights and the inclusion in the declaration and the four follow-up covenants of humanitarian provisions that her cautious superiors wanted excluded as troublesome nonsense. Unfortunately, Mower focuses almost exclusively here on Mrs. Roosevelt's leadership traits and thus doesn't tell us enough of what we'd like to know about the substantive issues and arguments that went on then and which still go on today whenever anyone suggests that the United States would be on firmer moral grounds as a human rights promoter if the Senate would finally ratify the covenants which have already been accepted by many of the governments whose human rights records are criticized by Washington.

In the second part of the book Mower skips ahead 25 years to the Jimmy Carter era (asserting, but not proving, that human rights were neglected by United States administrations during the 1950s, 1960s, and early 1970s). Mower shows that even in the last few years, though, security considerations and economic goals have taken priority over concern for economic/social and civil/political rights matters as even the Carter administration grapples with the many complicating and extenuating factors involved in each case in a necessarily case-by-case implementation approach. It is a thoughtful and balanced examination of the difficulties and dilemmas the policy makers must confront. Should the carrot or the stick be tried? Can the carrot be applied in a non-paternalistic fashion, the stick without obnoxiously interfering with sovereign prerogatives? Should aid be withheld because of a regime's poor human rights record even though it will be the people, not the government elite, who will suffer? But if aid is given, even if most of it actually gets to the peasantry, the odious government inevitably benefits and grows more secure. Mower seems to prefer the multilateral to the bilateral approach to inducing reforms; but he recognizes that the tradeoff involves further politicizing (and endangering) the many international banks whose charters forbid them from applying non-economic criteria when deciding upon loans.

A final chapter examines the efforts that have been made to institutionalize human rights concerns in the structure of the United States foreign policy bureaucratic machinery, efforts that Mower and other observers rightly commend as helpful: a new Assistant Secretary of State for Human Rights and Humanitarian Affairs has some clout and is charged with nagging his colleagues so that human rights concerns are at least considered alongside other concerns as the still-dominant United States acts, whether in bilateral or in multilateral channels.

JAMES A. STEGENGA

Purdue University

The President, the Budget, and Congress: Impoundment and the 1974 Budget Act. By James P. Pfiffner. (Boulder, Colo.: Westview Press, 1979. Pp. xii + 165. $17.50, cloth; $8.00, paper.)

The virtues of this book do not, alas, include additions to the stock of knowledge. It is a useful library-based study of events and trends that culminated in the 1974 statute. Two main threads are traced that were tied together in that act, in circumstances characteristically blending the fortuitous and the purposeful.

Two chapters accordingly review the development of executive domination of the budgetary process, and the emergence and spread of the practice of executive impoundments. The first sketches a familiar story. The second distinguishes chronological stages and widening goals of presidential impoundments, from Coolidge to Nixon, without classifying the oxen thereby gored. James Pfiffner spells out five differences between Nixon's actions and the precedents claimed for them, but does less than justice to LBJ's extensive impoundments. These, so far from embodying defiance of congressional wills, were pressed upon him by Wilbur Mills. The Tax Surcharge Act, finally passed in July 1968, commanded the president to impound billions more.

Two more chapters deal with the statutory and constitutional rhetoric advanced in behalf of impoundment powers; and with a new intervening element, a string of judicial decisions.

The next chapter treats the disappointments of past congressional efforts, back to 1946, to get control of the budget, and the breakdown of comity between Nixon and the Congress. Nixon's 1972 campaign demand for a general impoundment power to enforce an expenditure ceiling of his choosing was refused; instead, a congressional joint study committee devised what became the congressional budget bill. Meanwhile, umbrage over affronts to institutional prerogatives enabled Senator Ervin, from his Senate Judiciary Subcommittee on the Separation of Powers, to nurture a bill to channel and control impoundments. In the event, the political price of passing either bill through both houses proved to be hooking them together. The last substantive chapter recounts the formal side of this legislative history.

To an old editor it seems appropriate to notice the erratic manner of citing references. Acts of Congress are cited variously by Public Law number, by volume and page of the *Statutes at Large*, by title and section of the *U.S. Code* or (p. 106, n. 63) by a mixture. Quotations from public documents are cited to third-party intermediaries. This is well and good (p. 24, note 37) when Neustadt, in his classic article on central clearance, has picked up a passage of Roosevelt's comments buried in the minutes of the National Emergency Council. It is harmless but bizarre (p. 24, n. 43) when Rexford Tugwell's *Enlargement of the Presidency* (1960) is cited for a quotation from Roosevelt's message to Congress transmitting the Brownlow Committee report; at least Tugwell was around at the time. But it has misled the author into historical error (p. 13 and n. 18) to rely on Eisenhower's budget director, Percival Brundage, an accountant and surely a hearsay witness, in his book on *The Bureau of the Budget* (1970), for the statement that "in 1910 Congress passed a law establishing the Taft Commission on Economy and Efficiency. . . ." Taft established it, in 1911.

HARVEY C. MANSFIELD, SR.

Columbia University

Injury to Insult: Unemployment, Class and Political Response. By Kay Lehman Schlozman and Sidney Verba. (Cambridge, Mass.: Harvard University Press, 1979. Pp. xv + 393. $20.00.)

In the 1970s the study of political behavior moved from its base camp—the study of voting behavior—into territory that heretofore had been mostly *terra incognita.* Studies now began to map realms whose contours were defined exclusively by the speculations of political philosophers who had, to be sure, never trod the ground in arriving at their assumptions but who, after all, had theories. And these theories have come to guide empirical research. The results of the round of empirical explorations begun in the 1970s is *inter alia* a challenge to the theorists among us to adjust basic assumptions in light of the growing body of evidence on the links between the individual and the polity.

We now have available detailed mappings of the attitudinal effects of the encounter of the American citizen with federal bureaucracies; of the sources of presidential popularity; of nonelectoral political participation; and of the processes involved in the politicization of parochial concerns. With Kay Schlozman's and Sidney Verba's book we now also have a guide to understanding the political consequences of being economically deprived, in general, and being unemployed, in particular. Schlozman and Verba have drawn their map with such care and skill that we will all profit from studying their results and the process of exploration as well.

The study asks why being unemployed has no political consequences among the jobless. The underlying model is designed to account for this absence of politicization. Indeed, it describes the "steps by which those who have interested in common [on any problem] become an effective political force" (p. 12). The model includes five stages: (1) the existence of an objective condition or problem; (2) the experience of subjective strain caused by the problem; (3) the politicization of the strain via the perception of government as a source of solution to the problem *and* the perception that others share the problem as well–in other words, politiczation via "group consciousness" (p. 14); (4) the translation of this consciousness into political action via a shared political program; and (5) political relevance flows from this political activity, from individual and group mobilization and from the establishment of a political force seeking to relieve problem-borne strain. The model is not presented as a set of hypotheses about political process to be tested across a range of "objective conditions"–such a test would require comparisons among a set of problems. The model is assumed to be valid, and Schlozman and Verba track the attitudes of the unemployed to determine at which point the process of mobilization is interrupted.

The data for the study are drawn from two parallel telephone surveys of the metropolitan work force which yielded 1,370 respondents of whom 571 were jobless (Appendix A). The structured interviews were expanded upon in 60 follow-up depth interviews. On some questions, comparisons with data from extant surveys from the period of the Great Depression of the 1930s are used to suggest areas of attitude stability and change.

In the compass of this brief review I cannot adequately reflect the scope of the analyses undertaken, but I can portray the main outline of the findings with the intention of convincing the reader to study the book itself. Schlozman and Verba summarize their findings very carefully: "Objective economic disadvantage . . . produces strain." However, "there appeared to be little connection between personal economic condition and social ideology. This was seen most strikingly in the absence of a link between the severe personal strain of unemployment and cynicism about the American Dream or heightened class consciousness" (pp. 346–47). With the absence of this linkage, the political consequences of economic deprivation are substantially diminished. The deprived may look to government for aid, but they neither question the ideological foundations of American politics nor mobilize to wield political power within the existing system. If this lack of politicization of interest gives support to the contention of the "anti-pluralists" that the existence of a problem does not mean the mobilization of those with the problem, the polity's attempts to deal with economic deprivation supports the "pluralist" contention that representation of the unmobilized is a feature of the American system.

In the authors' view their study leads to a required rethinking of models of policy making based on the dialectic of interests. They conclude that in America the politics of belief is more vigorous than either political theory or studies of voting behavior have prepared us to expect. This conclusion will be controversial, but I do not expect much controversy over my contention that this book is an important sign of progress in the study of political behavior.

RICHARD A. BRODY

Stanford University

Letters of Louis D. Brandeis: Vol. 4, 1916–1921: Mr. Justice Brandeis; Vol. 5, 1921–1941: Elder Statesman. Edited by Melvin I. Urofsky and David W. Levy. (Albany: State University of New York Press, 1975, 1979. Pp. xxiii + 587; xxix + 770. $35.00, each.)

The publication of volumes 4 and 5 completes the project Melvin I. Urofsky and David W. Levy began in 1964: they have worked with painstaking care to gather all correspondence of Louis Dembitz Brandeis. As indicated earlier (*APSR*, September 1974, p. 1340), two caches were not available to them: the personal letters which Brandeis wrote to his fiancee Alice Goldmark and letters in the possession of Brandeis' former law firm, Nutter, McClellan, and Fish. The editors began with the principal Brandeis depository at the University of Louisville Law School, then turned their attention to widely disparate locations, including Zionist holdings in Israel.

To the political scientist concerned with judicial behavior, these final two volumes provide the potential for special interest, for they cover the years of Brandeis' nomination to the U.S. Supreme Court by President Woodrow Wilson and his distinguished career as a member of the "liberal bloc" under Chief Justices Edward D. White, William Howard Taft, and Charles Evans Hughes. To such a specialist, the volumes may well be a disappointment–as the editors themselves suggest–because Mr. Justice Brandeis reveals little of his own personality and that of interpersonal relationships within the Supreme Court. As Urofsky has stated in his biography, *A Mind of One Piece: Brandeis and American Reform* (Scribner's, 1971, p.

121), "On the bench, Brandeis stood as a model of judicial propriety. He would never discuss cases pending before the Court with an outsider, nor would he comment on decisions after they had been reached." In addition, he exhibited a highly developed sense of personal privacy which led him to destroy many private letters, even though he assiduously avoided discarding anything which touched his public career, before and after appointment to the Court. To portray his "non-official side," the editors have included many letters to his siblings as well as to his daughter Elizabeth.

About one-half of volume 4 is devoted to the confirmation battle over Brandeis' nomination to the Supreme Court. Through a prolific correspondence—four, five, six letters a day went out to the managers of his case in Washington alone—Brandeis took an active role in defending himself against the charges of those who opposed confirmation. Responding to the public opposition of former president Taft, he used the Senate battle once again to raise questions regarding Taft's personal indiscretion in the Pinchot-Ballinger affair.

If anything, the letters in volume 4 over-represent Brandeis' zealous concern with American Zionism such as his efforts in behalf of a Jewish homeland in Palestine and his subsequent loss of control over the movement in the early 1920s. The editors caution: "It is our belief that the non-Zionist concerns of Justice Brandeis are somewhat 'under-represented' by the selection of letters in the second half of this volume. Partly this is because of the simple fact that Brandeis lived in Washington now" (Vol. 4, p. xiv).

During his tenure on the Supreme Court, Mr. Justice Brandeis corresponded regularly with Felix Frankfurter. It is to Frankfurter that Brandeis reveals himself even while maintaining the purist's notion of sanctity and confidentiality of the Court's business. While this correspondence reveals much less of the inner works of the Court than, say, the exchange between Taft and his brother Horace, the reader is provided with bits and pieces which tend to confirm earlier research on Taft and Hughes Court behavior. For example, writing to Frankfurter on December 14, 1922, Brandeis explains that "when C.J. is in minority, the senior associate who is not is supposed to assign case, but, in fact, the C.J. is apt to do so" (Vol. 5, p. 79). Or, "The C.J. is incorrigible re Certioraris. But I guess we are over the worst of it for this season. He stands alone in his irresistible appetite" (Vol. 5, p. 243).

Brandeis' intense affection for Oliver Wendell Holmes shines through a correspondence that is largely devoid of personal feelings. Even though the letters offer a dearth of commentary regarding Holmes' contributions to important decisions, 1916–1932, one gains an appreciation for the bond between the two through such diverse and mundane concerns as Holmes' well-being during the installation of a new elevator in his home, walks together to an art museum, or even the knowledge imparted to Felix Frankfurter on October 28, 1926, that "Holmes J. was downed by a head cold, caught from Butler J. & absent from C[our]t yesterday on that account" (Vol. 5, p. 238).

From these final volumes, Brandeis' intense devotion to public service emerges as an important, if implicit, theme. Such devotion manifested itself in a number of struggles: progressivism in the development of American law, confirmation of the Court appointment, Zionism, judicial restraint, and acceptance of New Deal programs in the 1930s.

The editors have done an excellent job not only in deciding which letters to include, but also in writing helpful annotations which average five or six per letter. Volume 5 contains a very complete cumulative index.

ROBERT G. SEDDIG

Allegheny College

Unions in American National Politics. Graham K. Wilson. (New York: St. Martin's Press, 1979. Pp. xii + 168. $18.50.)

This book is primarily an attempt by a British political scientist to demolish misconceptions with which, he feels, many liberals, radicals and academicians judge American unions. It is not an in-depth study, but rather a brief survey of the national politics of American unions. And it is flawed in part because the author has failed to immerse himself sufficiently in the American political and labor milieu. This means that he is insensitive at times to key aspects of American government and that he overlooks certain relevant features of union involvement in national politics.

A central misconception Wilson seeks to dispel is that American unions are less deeply or generally involved in national politics than unions elsewhere. Throughout the book, Wilson stresses the similarities between American and British unions. Both are actively involved in national politics. Both are internally divided on certain issues: American unions on foreign policy, British unions on unilateral disarmament. Both are increasingly protectionist toward foreign trade. In concentrating on the similarities, the author fails to explore the major differences between American and British unions.

A second misconception is that American unions have regressed from a radical past and now represent a narrow, selfish, conservative force. Wilson contends that the image of a radical union movement in the 1930s reflects a gross misreading of history. Moreover, he points out that the AFL-CIO and certain nonaffiliated unions do fight for broad policy objectives—civil rights legislation and national health insurance—that benefit the disadvantaged. Even the foreign policy views of the AFL-CIO are not the epitome of reaction as charged by liberals and radicals. These critics have also erred, he argues, in stereotyping American union members as intolerant, bigoted and conservative.

Although American unions are pictured as having established a superb electoral organization and the best lobby in Washington, D.C., Wilson concludes they have been singularly unsuccessful in transforming narrow union objectives into national policy. Nor have unions captured national executive agencies that regulate industrial relations. Where unions have been successful, he says, is in more general policy areas, and then primarily as part of a coalition with liberal groups.

Unfortunately for American unions, Wilson contends, a gap has opened between liberals and the AFL-CIO. Unions are concerned with bread-and-butter issues and with protecting their industrial position. Contemporary liberals inside and outside of Congress focus primarily on protecting consumers and the environment and on improving the quality of politics and the operation of government. They have lost faith in the capacity of government to deal with the economic problems that concern labor.

Wilson offers no blueprint for reconciling liberals and labor. He hopes the replacement of George Meany and his political lieutenants will produce a union leadership better able to work with and more acceptable to liberal groups. Indeed, he attributes much of the blame for the fracturing of the old liberal-labor coalition to the unreasonable attitudes and misconceptions of American liberals. It is clear Wilson hopes that once liberals abandon their misconceptions, the falseness of which he has demonstrated, they will again be willing to establish an alliance with unions.

ABRAHAM HOLTZMAN

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Comparative Politics

Suburban Tokyo: A Comparative Study in Politics and Social Change. By Gary D. Allinson. (Berkeley: University of California Press, 1979. Pp. xv + 258. $18.75.)

Japan specialists know a good deal about villages, and are starting to learn how big-city politics works, but the suburbs—the fastest-growing sector, with one-third of the country's population—remain mysterious. Gary Allinson has picked two prototypical suburban cities to see how they developed over the past century. His main objective is to account for the rise of opposition party strength in the postwar period.

The two cities are both in the Santama area west of Tokyo. Musashino, closer in, became a suburb before the war; it has a concentration of well-educated white-collar commuters and the third-highest per capita income in Japan. Fuchū is less conveniently located and grew later. While it is also mainly white-collar, its population is younger, less well-off, and includes a significant proportion of locally employed factory workers. Interestingly, it is Fuchū which remains more attached to the conservative Liberal Democratic party: Allinson hypothesizes that its pattern of direct migration of less-educated workers from rural areas results in a less progressive political orientation than where, as in Musashino, many of the residents have moved out from the central city.

While this and several other propositions are intriguing, the book does not offer a coherent and convincing overall argument. Perhaps it has taken on some of the amorphous characteristics of the communities it describes. Allinson says he writes as historian, anthropologist, sociologist and political scientist, and it is hard to be all four at once. His cities do not provide a unifying historical story as compelling as the author's earlier account of an unfolding company-town relationship (*Japanese Urbanism*, 1975). The book lacks the rich ethnographic texture to convey an anthropological "feel" for the communities. It also fails to tell us enough about the farmers, old and new merchants, factory workers and various sorts of communters—their social characteristics, organizations, ways of thinking, interrelationships—to bring an understanding of the cities' social structures. And though the political analysis is more detailed, here too are data problems. The author believes that conventional survey techniques overlook group and community influences on voting choice (a reasonable observation, though I think he underestimates the potential of recent "social network" approaches). He therefore relies on illustrative examples and logical inference from aggregate voting data (25 tables) to see who votes for whom and why. Because these data are aggregated only at the city level, it is impossible to assess the accuracy of his interpretations, which in any case are not strikingly original.

Allinson has ambitious goals for this book and for the most part they are unfulfilled. Still, we do learn a lot about these cities, and about some important topics: the changes and continuities in candidate characteristics, party organization and electoral strategy; the subtle differences between elections at various levels, the ambiguous relationship between party control and local policy outputs. *Suburban Tokyo* should be valued as a venture into hitherto unexplored territory, and it does fill in some gaps in our understanding of political change in contemporary Japan.

JOHN CREIGHTON CAMPBELL

University of Michigan

Fluidità Elettorale e Classi Sociali in Italia: 1968–1976. By M. Barbagli, P. Corbetta, A. Parisi, and H. M. A. Schadee. (Bologna, Italy: Il Mulino, 1979. Pp. 182. Lire 5000, paper.)

Until recently, the Italian electorate was thought to be the most stable in Western Europe. Victory or failure for a party was gauged in tenths–sometimes in hundreds–of a percentage point shift. Italian voters were thought to be frozen into isolated Communist, Socialist, Catholic and lay subcultures. The nearly identical vote percentages for the parties in election after election seemed to confirm that interchange between the subcultures was unlikely.

Given these assumptions about the social basis of electoral stability, it is easy to see why the notable Communist gains in the 1975 elections and the move toward a two-party system in 1976 was seen by many observers as a "political earthquake," akin to a "critical election" Italian style.

The authors of *Fluidità Elettorale e Classi Sociali in Italia* use ecological data and regression analysis from six elections between 1968 and 1976 in the city of Bologna effectively to call into question this conventional wisdom. The authors attempt to show that beneath the apparent calm of Italian electoral results, massive voter shifts between parties have abounded. Until 1975, this "electoral fluidity" was disguised by the similarities in raw voter percentages, but the implications of fluidity are clearly drawn. Far from being an electoral earthquake, the 1975 and 1976 elections appear as a culmination of tendencies long festering in the political behavior of Italian social classes.

What have been the patterns of inter-party voter movement, and how do these patterns tie up with changing class support for the major parties? The authors try to demonstrate that the 1972 election—and not the 1975 or 1976 ones—was the high point of voter switching. In response to the collective protest of the late sixties and early seventies among the industrial working class and students, classes were increasingly polarized along a left-right continuum. The Communists collected former Christian Democratic workers, and ceded with the Socialists some of the lower-middle and middle-class electorate to the center and to the Christian Democrats. Contrary to generally accepted hypotheses, the authors show (at least for their sample) that the two major parties became more, not less, socially homogeneous between 1968 and 1976.

Perhaps the most startling finding of this work concerns the response of the Communist party to the class polarization evident in the 1972 election. Fearing such polarization as an incipient coalition of the right, the Communists began to *moderate* their class appeals, offering explicit and implicit alliance appeals to specific strata within the middle class. According to the authors, this strategy paid off electorally in 1975 and 1976 in the Communists' recovery among certain lower-middle class strata, even while the Christian Democrats consolidated their upper- and middle-class constituencies. In the process, new contradictions and problems were to plague the Communists, derived from a political strategy that ignored the influx of newly radicalized working-class and youthful voters in favor of concessions to the middle class.

Because of the careful yet fundamental way in which this work reformulates long-held notions about the Italian party system and its social base, *Fluidità Elettorale* is as significant for Italian political studies as Burnham's *Critical Elections* is for the U.S. While some may dispute the methodological assumptions of the work or the choice of Bologna as a case, the authors are careful to emphasize the contingent nature of their findings. While the book is enormously suggestive, it does not break new ground in political analysis. Given the obvious talents of these scholars, it is surprising that not more is said about the impact of party strategies upon electoral fluidity, or about the implications of "electoral fluidity" and class realignment for possible political solutions to Italy's economic, social and political crises.

RAYMOND SEIDELMAN

State University of New York, Albany

Soviet Law After Stalin: Vol. 1, The Citizen and the State in Contemporary Soviet Law; Vol. 2, Social Engineering Through Law. Edited by Donald D. Barry, George Ginsburgs, and Peter Maggs. (The Netherlands: Sijthoff and Noordhoff Publishers, 1977 and 1978. Pp. xv + 303; xiv + 335. $48.00, each.)

The increasing importance of law in Soviet society is reflected in these two volumes by many of the leading specialists in the Western world. It is really indispensable to any serious student of Soviet law. However, while the volumes incorporate pieces representing most of the major analytical approaches to Soviet law, it is notable that no really serious consideration of the rule of law in Soviet society appears. Despite the growth of scholarship on Soviet law, this is still generally verboten among students of the Soviet legal system; not, it might be suggested, for lack of evidence that there is an expanding sphere of law.

Going further, there is little attention given to the possibility of some organic institutional processes envolving outside the strict black letter of Soviet law, as has been the case with the enhanced lawmaking functions of the Presidium of the Supreme Soviet, now headed by Brezhnev. Perhaps the promised third volume will address these institutional issues more fully.

Specialists on Soviet law, including the Soviets themselves, tend to view law primarily as an instrument of socialization and mobilization, and are rather disinclined to take it seriously as a restraint on arbitrary rule. Robert Sharlet's piece is a particularly sophisticated exposition of this view.

The role of procedural due process is given little credence in a fascinating analysis by former dissident legal specialist, Valery Chalidze, now in exile in the United States. However, Donald Barry concludes that the sphere of law is expanding in more mundane areas like education and housing law, where the central ideological tenets, which bulwark the party-state apparatus, are less threatened.

There is little indication in any of these works that the rule of law has emerged as a factional issue within the leadership, although this has received some serious attention elsewhere; and is of special interest to political scientists, since it would represent a fundamental challenge, however embryonic, to the nature of the system itself.

Two quite valuable chapters on the new "Brezhnev" constitution by John Hazard and Yurii Luryi provide essential background for all the articles. All the authors did have time to consider the constitution before writing their papers.

Following is a representative sample of the pieces contained in the two volumes. "A Constitution for 'Developed Socialism,' " by John N. Hazard; "The New Constitution of the USSR from Draft to Law: An Analysis of the Changes Adopted," by Yurii Luryi; "Human Rights in the New Soviet Constitution," by Valery Chalidze; "The Soviet Union, 'Jus Comercii,' and International Law: The Case of the Most-Favored-Nation Clause," by George Ginsburgs; "Improving the Legal Mechanisms for Economic Change," by Peter B. Maggs; "Automobilization and Soviet Law: Reflections on Certain Aspects of Soviet Automobile Law," by Christopher Osakwe; "Consumer Product Quality in Soviet Law: The Tried and the Changing," by Zigurds L. Zile; "Law and the Delinquent Family: Reproduction and Upbringing," by Peter H. Juviler; "Administrative Justice and Judicial Review in Soviet Administrative Law," by Donald D. Barry; "Procuratorial Supervision of Economic Violations in the USSR," by Gordon B. Smith; "Crimes Against the Central Planner: 'Ochkovtiratel'stovo,' " by Stanislaw Pomorski; "Legal Policy under Khrushchev and Brezhnev: Continuity and Change," by Robert Sharlet.

PETER VANNEMAN

University of Arkansas

Politics in the Middle East. By James A. Bill and Carl Leiden. (Boston and Toronto: Little, Brown, 1979. Pp. xv + 416. $7.95, paper.)

Politics in the Middle East is an attractive addition to the Little, Brown series in comparative politics. It updates and replaces the authors' *The Middle East: Politics and Power* (Allyn and Bacon, 1974), and has new chapters on "The Arab-Israeli Connection" and "The Power of Petroleum." Whereas most Middle East textbooks are structured within a country-by-country or a problem-by-problem context, these authors have established a theoretical framework within which they attempt to fit their rich documentation. The new chapters on the Arab-Israeli conflict and on Middle East oil are useful for their information, but they do not fit the theoretical or historical framework of the rest of the volume.

The initial discussion of modernization and political development follows conventional lines with references to C. E. Black, James S. Coleman, Lucian W. Pye, etc. It includes a typology for Middle East development strategies which divides the countries of the region into: popular participatory, authoritarian-technocratic-traditional, authoritarian-technocratic-distributive, authoritarian-technocratic-mobilizational, and traditional. Although this is a somewhat arbitrary division, it helps to establish theoretical parameters for a comparative study of the region's political institutions.

A provocative aspect of the study is the chapter, "The Politics of Patrimonial Leadership," where the authors adapt Weber's analysis to Middle East history and contemporary political development. Their application of Weberian categories such as charisma, patriarchial and patrimonial systems, the politics of personalism and relations of emanation to Middle East history and politics is successful, if at times forced. Indeed, themes such as the importance of physical proximity, informality in personal politics, balanced conflict, and charisma, are not unique to the Middle East. Their application here demonstrates the similarities to political development elsewhere. The themes and categories developed in this chapter form the matrix of analysis for the rest of the book. This theoretical framework enhances discussion of recent political developments such as those described in the chapter on "The Politics of Leaders and Change." The latter includes a timely and informed analysis of the decline and fall of the monarchy in Iran and of the transition from Nasser to Sadat in Egypt.

Chapters on "Violence and the Military," and "The Imprint of Ideology," illustrate the difficulties in attempting to apply broader theoretical constructs to the Middle East, or in developing new theoretical generalizations about the area. Generalizations that emerge about violence, the military, and the role of ideology in the region are not unique. Its historical background, especially the matrix of Islam, and continuation of that history into the present, make possible a more powerful and viable explanation of contemporary Middle East politics than do some of the newer theories of modernization. The chapter on "Islam and Politics" is more illuminating than the chapters on modernization and patrimonialism for guidance to current political developments in the region.

The authors know their history, know it well and use it with grace and style. Although at times the theoretical discussion may seem like pouring old wine into new bottles, any shortcomings in the theoretical discussion are redeemed by the authors' extensive familiarity with the present situation in the Middle East and by their sense of history. They demystify, demytholigize, and "de-demonize" the region. The chapter-by-chapter bibliography and the footnotes which refer to official documents and periodical literature add to the book's usefulness both as a text and a reference work.

DON PERETZ

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Moving a Mountain: Cultural Change in China. Edited by Godwin C. Chu and Francis L. K. Hsu. (Honolulu: University Press of Hawaii, 1979. Pp. ix + 446. $17.50.)

For several years, Godwin Chu and Francis Hsu, both affilated with the East-West Center at Honolulu, Hawaii, have conducted symposiums to evaluate social and political changes on mainland China since the Communist takeover in 1949, and have published the papers presented under their joint editorship. *Moving a Mountain* presents the result of one of these symposiums which focused on the role of communication in the process of cultural change in China since 1949.

The book consists of 16 papers grouped into four sections: (1) Communication System, (2) Political Culture, (3) Value Change, and (4) Conclusions. The conceptual framework of cultural change and communication is set forth by Godwin Chu in the introduction. At the end, the editors sought to integrate the papers into a coherent whole in terms of Chu's conceptual framework.

It is difficult to evaluate this book since, as is true of so many collections of this kind, the quality of the papers is extremely uneven and the "conclusion" is ambiguous at best. To begin with, Godwin Chu has written a very perceptive introduction on the concept of cultural change, which he defines as significant changes in three sets of relations: (1) between the self and significant others, (2) among the self, materials and objects, and (3) between the self and ideas. But his concepts pertain almost entirely to "culture" with little discussion of "communication" (in one instance he regards "culture" and "communication" as one and the same). Furthermore, Chu's conceptual framework is too broadly construed and is almost entirely unconnected to the 16 individual papers which constitute the body of the book.

In the section, "Communication System," two papers are more informative and insightful than others. They are a study of local newspapers in China by Lynn White III and Martin Whyte's study on the use of "small groups" for persuasion in China. Both show how "practice" deviates from "theory" and serve to give us a more complex and accurate picture of communication in China than party propagandists would have us believe.

In the section on political culture, three stimulating papers are Lucian Pye's interpretation of Chinese political culture as seen from the way Chinese leaders use mass media, Lowell Dittmer's insightful psychoanalytic study of the impact of the Cultural Revolution on various groups of Chinese, and Parris Chang's thematic analysis of 175 picture storybooks used by Chinese children as a way to generate hypotheses about Chinese political socialization.

But, in my opinion, the two best papers of the whole symposium are: "Peasant Culture and Socialist Culture in China" by Vivienne Shue, and "Communication and Changing Rural Life" by William Parish. Through a very careful and ingenious analysis of official reports, Shue is able to make explicit Chinese Communist leaders' implicit notion of bringing value changes to the peasantry. First, Chinese leaders sought to induce peasants to adopt new behavior on the ground of realizing their *old* values. Then once the new behavior (e.g., collective farming) was adopted, the leaders hoped for peasants' accepting new (socialist) values to institutionalize the new behavior. But as Shue points out, in practice Chinese Communist cadres at the grass-root level seldom ventured beyond the first step, i.e., enticing peasants to adopt new behavior in terms of realizing their old values. William Parish's paper presents some of the major findings of a large survey project (done with Martin Whyte) with some former rural residents of 63 villages in Kwangtung province. Parish and Whyte use statistical measures to study the correlations between communication behavior (newspaper reading, movie viewing, listening to broadcast and attending meetings) and "village altruism" (as indicated in "distribution systems," "collective education," "medical system," and "conflict") and between communication behavior and "rural change" (as indicated in "collective life," "family life," and "ceremonial life"). With one or two notable exceptions, Parish finds no significant correlation between communications and "village altruism" and "rural change." Parish concludes that lack of such correlation does not mean that the media fail to reach the peasants. The media do reach the peasants in Kwangtung, but the peasants "responded less to messages being communicated from Peking and less to the political consciousness or socialist character of each village than to the mixed structure of incentives created by new collective structures established in the 1950s" (p. 379). Thus, Vivienne Shue's careful qualitative analysis corresponds closely with the survey study by Parish and Whyte.

At the end of this symposium Doak Barnett sums up the pattern of communication in China today and suggests new areas for further research. Barnett's essay is a "must reading" for all China specialists interested in communication and development in China.

In addition to the criticism mentioned earlier that the conceptual framework set forth by Godwin Chu fails to connect all the papers, another can be made: the book could have been more informative if the two editors had used comparative analysis. There is a rich literature on what kinds of revolutions typically do or do not succeed in accomplishing their manifest tasks. Neither the editors nor most of the writers of this book show sensitivity to the need for setting the Chinese experiences in a comparative perspective.

ALAN P. L. LIU

University of California, Santa Barbara

Die Planungszelle. By Peter C. Dienel. (Opladen, West Germany: Westdeutscher Verlag, 1978. Pp. 276. DM 23.)

The demand for greater involvement of citizens in public policy formulation has been rising in West Germany as well as in other post-industrial countries. As a result of this increased interest, as well as the influence of *Burgerinitiaven* (Citizen Action Groups) in German politics, a number of German scholars have begun studying and writing about new modes of citizen participation.

Peter Dienel's book presents one model for organized participation, called the *Planung-*

szelle. A randomly selected group of 25 to 300 is brought together to work on a specific policy question for three weeks under the guidance of an outside facilitator. Their solutions or recommendations are then presented to the decision-making body, usually a city council or *Land* parliament. During this three-week period the state pays the regular salary of those involved.

This system of requesting citizen input into policy decisions has been tried in Bavaria, Solingen, and most recently, in Cologne. In Cologne the *Planungszelle* was assigned the task of recommending urban renewal plans for the inner city area located near the City Hall. After more than 400 meetings, the 250 citizens who were involved decided to enlarge the city hall square and to construct some low-rise housing behind the city hall. This proposal countermanded the prize-winning plan of a famous city planner who had recommended restoring the city hall square to its seventeenth-century grandeur. Cologne's chief municipal planner had originally favored the open-square plan recommended by the *Planungszelle.* This might lead a critic to question the method of providing the information and the role of the facilitator.

A more important criticism of the efficacy of the *Planungszelle,* in addition to the opportunities for the manipulation of the results, is the restriction of the input into decisions by other citizens, some of whom may be more directly affected by the issue under consideration. Not only the *Planungszellen* but all citizens should have an opportunity to learn participatory skills that would enable lay people to evaluate governmental programs and provide a year-round check against the increasing professionalization of public services.

Increasing bureaucracy and technocracy have steadily reduced citizen participation in decision making. Dienel's book is representative of attempts in West Germany to find new solutions to the rigidity of post-industrial systems. It also provides a good bibliography of the growing literature on "organized" participation in administration and governance in Europe. What is needed now is a study of the relative merits of both organized and spontaneous participation. Are these contradictory modes, or can a combination of the two be used for effective participation?

CHARLES R. FOSTER

Committee on Atlantic Studies

Doctrine and Ethos in the Labour Party. By H. M. Drucker. (Boston: George Allen and Unwin, 1979. Pp. viii + 134. $17.50, cloth; $7.50, paper.)

Doctrine and Ethos in the Labour Party is a fine book: insightful, succinct and a delight to read. It is not a comprehensive or detailed study of the Labour party. Rather, it attempts to introduce a new distinction—ethos as opposed to doctrine—and to pull together the various findings of other scholars. It succeeds in both tasks.

Drucker argues that a party's ideology actually has two dimensions: the party's doctrines and its ethos, which he defines as the "traditions, beliefs, characteristic procedures and feelings which help to animate the members of the party" (p. 1). The ethos of the British Labour party emerges out of "the experience of the working class" and consists of "a series of folk-memories" or "shared expressions of exploitation, common struggle, and gradually increasing power" (pp. 31–37). That experience, Drucker tells us, "can be summed up for brevity's sake, in two aspects: exploitation by outsiders, and the slow, painful building of its own defensive organizations" (p. 19).

The defensive ethos of the Labour party manifests itself in numerous ways. Most generally, it has led to caution, incrementalism and excessive reliance on "tried practices." Specifically, Drucker identifies four "expressions" of the party's ethos: "an unwillingness to sack leaders" for fear of being divided and more easily exploited; "the way it hoards its money," which derives from the thrifty habits of the British working class; "the formality of its practices" as a consequence of the desire to create "its own self-centered quasi-legal rule system" to offset the "alien legal structure" of bourgeois society; and the "demands for sacrifices from its leaders" as proof that the leaders have not betrayed the party by becoming middle-class.

Drucker then considers conflicts between the party's working-class ethos on the one hand and its revisionist and parliamentary elements on the other. Revisionist doctrine challenges the party's ethos in several important ways. The proposal to do away with "clause 4," for instance, ignores the symbolic value of nationalization to the working class. The suggested substitution of egalitarianism for state ownership of the means of production, to take another example, runs counter to the trade unions' principle of "procuring their differentials over those groups immediately beneath them, or trying to prevent any widening of the differential between their own group and the next above" (p. 62).

The party's ethos also causes tensions between the parliamentary and non-parliamentary wings of the party. Manifestoism—binding candidates to carry out the provisions of the election manifesto—is one symptom of this tension. Candidates are saddled with policies they cannot implement in office, thereby dashing the "exaggerated hopes" of the electorate while providing unreconstructed activists with exemption from blame. More importantly, there has been a growing rift between those who must bear the responsibility of governing and those who do not. Recent Labour governments have had to back away from socialist planning and egalitarian programs for political and economic reasons, opening up "for the first time in the history of the party, a gap between those who operate within the terms of the party's ethos, the constituency parties, and those who march to new tunes" (p. 111).

Drucker's insights are by no means uncontroversial. There is no attempt to measure or provide social scientific evidence of the phenomena he describes, and hence no way of telling how pervasive the working-class ethos is inside the Labour party. Other explanations of the behavior he describes cannot be easily dismissed: caution, incrementalism and resistance to change are typical features in bureaucracies, and the trade unions and the Labour party are certainly bureaucratic organizations. However, these are presumably the sorts of controversies Drucker intended to provoke.

BRUCE E. CAIN

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Politics and Policy in Australia. By Geoffrey Hawker, R. F. I. Smith and Patrick Weller. (St. Lucia, Queensland, Australia: University of Queensland Press, 1979. Pp. xii + 313. $24.25, cloth; $14.50, paper.)

In adapting to the newly emerging concern with public policy as a field of study, political scientists seem to be moving reluctantly and with great difficulty away from their traditional focus on political institutions and processes. Public policy is after all a product of those institutions and processes. Therefore, to the extent the study of public policy concentrates on the institutions and processes by which policy is made rather than on the policies themselves, the study of public policy becomes indistinguishable from the study of government and politics.

The book reviewed here focuses on the policy-making institutions and processes in Australia, specifically eschewing any attempt to analyze or evaluate policy content. The format is first to examine in some detail major institutions and actors in the policy-making process and then to examine five Australian case studies: school funding, the introduction of wheat quotas, the scope of the Department of Foreign Affairs and its relations with other departments, the development of the Royal Commission on Australian Government and Administration, and making the 1974 budget.

Much of the book is a descriptive narrative. What coherence and analytic quality the book has centers around supporting the imprecise proposition that policy making in Australia is more complex, more political and more incremental than a rational ideal based upon a clear separation of politics and administration. The authors strongly suggest that this rational approach to policy making is widely assumed to describe the British type of system, what the authors call "the Westminster Model." Of course, as the literature on policy and administration makes abundantly clear, this rational ideal is not a uniquely British phenomenon but may apply equally well to a wide variety of other systems.

The serious question is how widely this rational ideal is assumed to apply to a great extent anywhere. One may doubt that the news that policy making in Australia is influenced by considerations of politics, self-interest and existing policy will come as a startling revelation. The rational model and Lasswell's sequential model (p. 18) become straw men for the book. In this sense, it may be argued that the principal accomplishment of this detailed description of numerous episodes in Australian political life is to translate what was already well established generically into Australian proper nouns. Thus, for instance, we are told that federal funding of primary and secondary education was preceded by and partially legitimated by less "revolutionary" steps of funding universities and secondary school libraries. The point of this is to support the authors' contention that "incrementalism is characteristic of most policy-making processes." This detailed description does little to test the hypothesis of pervasive incrementalism, however. By the authors' own admission, the term "incrementalism" lacks the necessary precision for such a test (p. 162).

The most troubling aspect of books such as this is that they constitute old goods in new packaging. Explicitly eschewing concern for the content of policy outcomes either as dependent or independent variables (cf. p. 205), the authors are left with a discussion of Australian institutions and politics that is no more a "policy" book than the countless other books on the governments and politics that produce policies, especially those concerned with the administrative sectors. The narratives of particular decisions or processes in this book are thorough and well written. What this book fails to do is to show how the study of the policy process is distinct from a general study of government and politics, or how that policy process in the Australian context differs from widely known policy-making patterns everywhere else.

LAWRENCE MAYER

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The Christian Democratic Parties of Western Europe. By R. E. M. Irving. (Winchester, Mass.: George Allen & Unwin, 1979. Pp. xxii + 338. $35.00.)

The elections to the European Parliament held in June 1979 demonstrated again the appeal and strength of Christian Democracy in Western Europe, even under the banner of the new trans-national European People's Party. This federation of national parties which ran candidates in six of the nine Common Market countries succeeded in receiving more than one-fourth of the seats in the new assembly, placing it only slightly behind the Socialist bloc. In five of the member-states of the European Economic Community—Italy, West Germany, Belgium, the Netherlands and Luxembourg—Christian Democratic parties have been dominant or major political forces since 1945, averaging 36 percent of the vote in the general elections of the late 1970s. If one includes France, with a much smaller but not unimportant Christian Democratic tradition, the figure is reduced to 28 percent.

R. E. M. Irving has written a timely and very useful book about Christian Democratic parties in these six countries (not, as the title suggests, about Christian Democracy in all countries of Western Europe). His well-informed, current, and sympathetic yet not uncritical review of individual parties covers prewar developments, but the focus is on the post-1945 era. The analysis ends at about mid-1978; thus it includes the 1978 French parliamentary elections but not the Italian elections or the elections to the European Parliament in the summer of 1979. Nor, at the time of writing, had Franz Josef Strauss of the German CSU been elected the 1980 candidate for the chancellorship representing the combined CDU-CSU Christian Democrats. However, this event would probably have had little effect on Irving's conclusion that "the chief weakness of the CDU-CSU is that it is two parties, not one" (p. 256). An important theme of the book is the fluctuating fortunes of the Christian Democrats whose strong or even dominant positions were challenged and weakened in the 1960s, whose electoral support improved rather dramatically in the latest elections of the 1970s, and whose organizations appear to have experienced considerable rejuvenation in recent years. Election results and participation in postwar governments are reflected in very useful tables.

The selection of only some parties for inclusion in this book proves to be both a strength and a weakness. It is a strength, because the most important Christian Democratic parties in the largest and most populous continental countries of Western Europe are described and analyzed in considerable detail. This gives Irving sufficient opportunity to compare the basic similarities as well as some important differences among these parties in terms of history and cultural setting, ties to the Church, orientation toward left and right, organization, electoral appeal and leadership style. Though each country is treated in separate chapters, frequent references are made to similar or contrasting situations in other countries in these and other chapters in which the author integrates and synthesizes his numerous findings and conclusions. There are few authors whose combinations of scholarship and comparative approach can match Irving's contributions to the literature on comparative political parties.

It is therefore disappointing that other Christian Democratic parties, e.g., those in Austria, Switzerland, Spain, Portugal and in the Scandinavian countries, were not included in the analyses. I would hope that the Royal Institute of International Affairs, for which this study was written, will now ask Irving to keep the promise contained in the title of this book and complete a second volume on Christian Democratic parties in those countries of Western Europe that are not members of the EEC and the European Parliament. It would certainly help, though, if the price of the book could be set a bit lower.

ARTHUR B. GUNLICKS

University of Richmond

Ethnicity and Equality: The Shiv Sena Party and Preferential Policies in Bombay. By Mary Fainsod Katzenstein. (Ithaca, N.Y.: Cornell University Press, 1979. Pp. 237. $15.00.)

The central theme of this work can be easily stated: under what conditions does emergent nativist sentiment become organized into a successful political organization? In addition, the study challenges an assumption of comparative theory that individual merit and achievement are the basis of personal advancement as societies become more modernized. As such, the book admirably documents the rise of the Shiv Sena (i.e., Army of Shivaji, the seventeenth-century Mahratta hero) movement as one pressing claims for preferential treatment of its Marathi-speaking constituency and at the same time paints a revealing picture of a small regional political party so typical of India's complex and fascinating party system.

At the outset of the volume, Katzenstein distinguishes between the "old ethnicity," i.e., demands for regional autonomy which might threaten national integration and the "new ethnicity" or sons of soil movements such as the Shiv Sena which demand preferential treatment for their members in the way of quotas in jobs and educational institutions. In the case study, the author examines several factors related to the rise of organized ethnic conflict in Bombay. These are in particular migratory patterns, economic inequalities and competition, and party organization and leadership. For reasons peculiar to Bombay's colonial status and its position as the major western port, its population has always been ethnically diverse and heavily migrant; after the industrial era began, outsiders predominated in commercial and industrial occupations to the exclusion of Maharashtrians and at the same time maintained control of the city's politics. Even at present there is a high influx of migrants from other areas coupled with low out-migration. Since this pattern is typical of other urban areas in India, the author concludes that a demographic explanation is not in itself sufficient to account for Shiv Sena's success.

Middle-class Maharashtrians felt most slighted in competing for white-collar jobs, many of which were held by South Indians. The latter group, although outnumbered by Gujaratis, became the principal target of Shiv Sena protests largely because of their more visible cultural differences. While economic conflict was a major factor in middle-class support, Shiv Sena's electoral success relied on mobilizing the working-class voters as well. Here, the Shiv Sena had less success. Even though the party's right-wing appeal did have some measure of popularity among the working class, and its efforts at providing amenities in slum areas gained it favorable recognition, economic pressures were much less than among the middle classes, and many continued to support the Congress party.

The author attributes much of the party's effectiveness to its highly disciplined, paramilitaristic organization which engendered a vibrant sense of enthusiasm among its young, upwardly mobile activist cadres, as well as to the widely circulated Marathi language weekly *Marmik*, edited by its leader and founder Bal Thackeray. She is more cautious in assessing the impact of government policy on the actual improved conditions of Maharashtrians in Bombay. Even though the state government more than the municipal council supported its demands, it is suggested that official policy may have strengthened existing trends which were already in effect before the dramatic appearance of the Shiv Sena.

After their first electoral success in 1968 (electing 42 out of 140 city councilors), party leadership found itself in a dilemma typical of so many other regional parties in India: how to expand its electoral base. To do so it became necessary to mute its denunciation of other ethnic groups, and went so far as to support a South Indian in the 1971 parliamentary elections. Perhaps the one weakness of this otherwise excellent study is the failure to analyze in more detail the impact these ideological shifts (and other difficulties briefly cited) on Shiv Sena, which led to a decline in its electoral support (by 1978 its representation on the city council had been reduced to half its initial strength).

Nevertheless, Katzenstein's conclusions suggest propositions which are indeed promising and obviously lend themselves to testing in future studies. Although nativist sentiment and perceptions of ethnic inequality might be widespread, she argues that at least three other conditions must be met before a political movement of consequence can be effectively organized: low out-migration, middle-class economic competition and superior party organization with emphasis on mobilizing the youth. If Katzenstein's arguments prove correct, we should expect to witness similar ethnic movements demanding preferential treatment in similar environments. In sum, this is a concise, entirely readable volume which undoubtedly will appeal to those whose interests lie in the direction of ethnic politics as well as to South Asian area specialists.

DANIEL R. GRAVES

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The United Arab Emirates: Unity in Fragmentation. By Ali Mohammed Khalifa. (Boulder, Colo.: Westview Press, 1979. Pp. xvi + 235. $24.50.)

This comprehensive work on the United Arab Emirates (UAE) deals with the background and political development of the new nation as an integrative venture. In 1968 Great Britain declared its intention to end its imperial presence in the Trucial States in 1971. Following three years of negotiations, Abu Dhabi, Dubai, Sharjah, Ajman, Umm al-Guiwain, and Fujairah established the UAE as a federal system on December 2, 1971; Ras al-Khaimah joined the union two months later.

Much attention is devoted to factors that help or hinder political integration. "The quest for survival of the status quo, rising standards of living, the existence of a core unit, gradualism, the sense of mission, geographical contiguity, and cultural commonality are elaborated on as primarily integrative factors" (p. xv).

The paramount position of Abu Dhabi in the union is viewed as a positive force for stability and integration. The role of this emirate is seen as somewhat analogous to that of the United States in NATO, or that of the Russian SFSR within the Soviet Union. It is pointed out, for example, that Abu Dhabi contains over 86 percent of the union's total area and 40 percent of its population. Because of its oil, it is by far the richest member of the UAE, and the emirate consistently provides over 90 percent of the federal government's finances. The UAE came about largely because of the initiative of Abu Dhabi's talented ruler, Shaikh Zayed. Since independence, the emirate has exercised a benevolent, hegemonial control of the nation. "Abu Dhabi has invested a great deal in the federation, in both skills and treasure, and surely would opt not only for its preservation but for its growth and consolidation as well" (p. 126).

Chapter 3 is concerned with the structure and functions of the federal government. Although the formal institutions of the government are rather complex, the center of power is the Supreme Council of the Union (SCU). Here, despite the facade of a modern institution, the traditional pattern of elitist-hereditary rule persists. The SCU consists of all the rulers of the member emirates. For meetings to have legal effect, at least five of its seven members, including Abu Dhabi and Dubai (the second most important emirate), must be present. Each emirate has one vote in the council, but the votes are not of equal weight. Any important decision requires a majority of five, including the two votes of Abu Dhabi and Dubai.

The UAE is, of course, a major oil exporter. Among Arab oil-producing countries, it ranks fourth, being outproduced only by Saudi Arabia, Kuwait, and Iraq. Because of this and, more broadly, because oil makes the Persian Gulf region one of the most strategically important areas on earth, many readers will find chapter 7, "The Regional and Global Contexts: Support and Disruption," one of the most useful parts of the book. Of particular interest is the author's study of the background and dynamics of Arab radicalism in the region. Included is a discussion of the Popular Front for the Liberation of the Occupied Arabian Gulf (PFLOAG), its Marxist-Leninist ideology, and its linkage to extra-regional elements.

Khalifa tells how the Dhufari rebellion was checkmated largely because of military aid provided to Oman by the Shah of Iran. In general, the United States and other Western nations benefited during the years when imperial Iran played the role of "gulf policeman."

In his concluding chapter, Khalifa notes that the UAE Provisional Constitution expires in 1981. He speculates that integrative achievements may lead to the abandonment of federalism in favor of a unitary system. He also observes that political, economic, and social development may lead to a demand for some form of democracy. Will the "leadership be able to cope with increasing popular demands for wider political participation?" (p. 181).

ROY E. THOMAN

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Doing Good by Doing Little: Race and Schooling in Britain. By David L. Kirp. (Berkeley: University of California Press, 1979. Pp. xi + 164. $11.95.)

In the last 20 years, British and American policy makers have adopted quite different strategies for remedying racial discrimination in education. While Americans have attacked it head-on through direct legislation and adjudication, the British have preferred not to confront it directly, but only as part of the broader issue of educational disadvantage. David Kirp's short account of this British policy of "racial inexplicitness" addresses the debate over race in both countries. But the book suggests particularly interesting possibilities for American policy by showing how a form of "benign neglect," which this country rejected almost out of hand in 1970, has worked in the British context.

Although inexplicitness has in certain instances meant "doing nothing," Kirp argues that in Britain it has also meant "aiding nonwhites with seeming inadvertence, doing good by stealth" (p. 105). English-language instructional programs, for example, are earmarked for students with language difficulties, a disproportionate percentage of whom happen to be nonwhite immigrants. Kirp presents data to demonstrate that inexplicitness has in fact coincided with a period of some improvement in conditions for Britain's nonwhites. Avoiding specific policy pronouncements, he draws the general conclusion that inexplicitness may provide the sort of "finely tuned and ad hoc" policy which is needed to cope with that "disadvantage more nebulously linked to race and more clearly tied to economic circumstances" which occurs increasingly in both Britain and America (p. 123).

This balanced and readable case study offers additional evidence that policy making in Britain is largely informal, consensual, and nonlegal; and it brings valuable comparative perspective to an important issue in American politics. American readers will find the chapter on school busing especially informative. But political scientists—particularly those familiar with Britain—may feel the author has taken too narrow a view of his subject by paying insufficient attention to the politics of race.

In his defense, one must admit that until recently Britain's limited national debate over race had focused mainly on immigration policy, not education—leaving the latter largely in the hands of bureaucrats in the Department of Education and Science and in the local education authorities. Still, Kirp's inattention to party politics may have led him to underestimate the level of dissensus over race which actually exists in Britain.

The Labour and Conservative parties, for instance, probably face greater pressure from the National Front than he recognizes. Although it is true that this racist movement "has attracted more attention than votes" (p. 111), it managed to take 16 percent of the vote in the West Bromwich by-election (1973) and competed well enough at Stechford (1977) to force the Liberal party's candidate into fourth place. Given the sorry state of their constituency organizations, the Front could also draw votes away from candidates of both major parties, especially in municipal elections. To the extent, then, that it succeeds in politicizing race in Britain, the National Front renders a policy of racial inexplicitness more difficult to sustain. That fact may help explain why British policy makers have appeared more willing since 1974 to single out race for special attention, and it forces one to wonder whether racial inexplicitness could ever be grafted onto race and schooling policy in America.

ELIZABETH H. CRIGHTON

Pomona College

Citizen Politicians-Canada: Party Officials in a Democratic Society. By Allan Kornberg, Joel Smith, and Harold D. Clarke. (Durham, N.C.: Carolina Academic Press, 1979. Pp. xiii + 243. $15.95, cloth; $8.95, paper.)

Given the voluminous literature on political recruitment, one approaches yet another such study with some trepidation. Fortunately, Kornberg and his colleagues have provided not only an interesting analysis of Canadian political recruitment, but also a work which provides significant substantive and methodological contributions which should positively influence future political recruitment research.

Utilizing both a "social structural" perspective (i.e., a person's social and political statuses are affected strongly by the statuses, skills, and resources one is born with and acquires subsequently) and a "political socialization" perspective (i.e., the importance of accumulated individual experiences), Kornberg et al. confirm the hypothesis that "party officials predominantly derive from a very small segment of middle and upper-middle class persons who have been reared, by average standards, in highly politicized milieux" (p. 217). In establishing the parameters of party involvement, support is demonstrated for the "Chinese Box" model of political participation and recruitment. Revealing is the political socialization data which suggest that "party officials may possess rather extensive information about the lives of people to whom they turn for assistance" (p. 76). The political recruitment process exhibits an important social dimension: "To a substantial degree it takes place within a network of personal relationships of varying closeness" (p. 76). An analysis of the affiliation process indicates that Canadian parties "appear to proceed with the business of acquiring new members much as do other voluntary organizations" (p. 104). As a result, the political recruitment data "suggest that Canadian party officials, as a group, diverge considerably from a model of committed politicians, consumed by politics, ideologies, and programs" (p. 93).

After establishing who joins local party organizations (Chs. 1–3), and analyzing the process and components of affiliating with a political party (Chs. 4–5), Kornberg et al. focus on the nature of party career patterns (Chs. 6–7). A threefold typology of party officials is developed: Stalwarts, Insiders, and Elites. The finding that similar proportions of each type exist within the various local party organizations suggests that Canadian parties are "primarily public office-seeking organizations" (p. 157). Application of the social structural and political socialization perspectives reveals significant differences in the status and politicization backgrounds of these three types of party officials. Consideration of the party careers of women (Ch. 8) finds that "their political socialization experiences not only are important for bringing women into parties, but also have consequences for some of the factors that directly determine whether they will rise to Elite and Insider statuses within them, or remain Stalwarts during the course of their careers" (p. 212).

The above findings are based on three sets of data: first, a study of 625 local party officials in Vancouver, British Columbia, and Winnipeg, Manitoba; second, a matched sample of nonparty members, who are termed "nigh-dwellers"; and third, a cross-section of 1100 members of the general population in these two metropolitan areas. While the usual criticisms of such survey research projects could be made, they are minor when compared to the advancement this study makes to the political recruitment area. The major methodological contribution is the use of a matched sample of the neighbors of party officials (i.e., the nigh-dwellers) for comparisons with the party officials themselves. The significant substantive findings include the following: first, social networks rather than ideological beliefs lead to the recruitment and participation of individuals in party organizations; second, both social structural factors and political socialization experiences influence various aspects of the political recruitment process; and third, once a person has joined a party, careers are likewise influenced by the individual's social background and the political milieux. In addition, this study provides some much-needed empirical data on and explanation of why women are a "disadvantaged majority" in terms of party participation. Finally, the utility of the political socialization perspective is demonstrated, a finding which should force a reconsideration of recent critiques which have sought to question the validity and reliability of political socialization research.

RONALD G. LANDES

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Is Britain Dying? Perspectives on the Current Crisis. Edited by Isaac Kramnick. (Ithaca, N.Y.: Cornell University Press, 1979. Pp. 286. $15.00.)

If Britain is indeed experiencing a life-and-death crisis, the essays in this volume provide little confirmation of it. These essays were first presented at a conference entitled, "The British Crisis: Real or Imagined?" held in 1978. As is typical of such collections, the papers lack thematic and theoretical coherence. Only an implicit assumption of a British civility in devising solutions is common to the participants. Perhaps most disconcerting are Edward Heath's assertion that British problems are solvable through greater international cooperation and Barbara Castle's contention that British ills are the fault of international forces. If politicians can be expected to offer reassurance, one would expect scholars to assess British destiny according to their own theoretical perspectives. Whereas other volumes are focused on the failure of social democracy or the difficulties in governing modern Britain, Kramnick hoped to assess its economy, its world role, and its ability to resolve conflict by democratic procedures, the "three salient features of its former grandeur." In fact, these categories are overpowered by the diversity of various authors' own interpretations, undermining any systematic basis for diagnosing whether Britain actually has contracted a fatal ailment or for explaining its symptoms.

Yet, the essays do provide basic information, identify some important trends, and, in several cases, offer stimulating analyses of the conundrum that plagues Britain. For instance, four interesting essays attribute responsibility for Britain's poor economic performance to key elites. Discounting Britain's long-term economic decline, Stephen Blank argues that decisions designed to maintain its international economic and political stature in the post-World War II era prevented the development of the domestic economy and that these decisions are linked to the ineffectiveness of Britain's political institutions. Robin Marris, to the contrary, argues that a relative economic decline cannot be played down. It is, rather, indicative of a basic technological and commercial backwardness, having its primary source in the failure of British society to produce talented managers. Richard Rosecrance, likewise, finds too little entrepreneurial innovation. Arguing that British power derived directly from its economic power, he contends that economic decline follows from an imperial dominance built on a surplus earned from invisible factors such as shipping, insurance, and financial services. This system, depending on financial resources and skills, undermines industrial and export competitiveness. Rosecrance sees greater strength in a dominance based on a surplus of merchandise trade. If only Britain would focus on exports and productivity, the future would be brighter. For Tom Nairn, Britain's imperial decline arises from a failure of political development: a product of the prolonged domination by a pre-modern financial elite who have stymied industrial development and modernization. He projects a breakthrough to modernity to coincide with the emergence of a right-wing national government, acting in the name of the public interest.

Other important essays deal with the disintegration of power centers. In a systematic defense of British political institutions, Jorgen Rasmussen details the loosening of cabinet and party control over the Commons, a change in behavior that reflects a weakening of class cohesion. Ira Katznelson accounts for the erosion of class cohesion in terms of the increasing separation of workplace and residential communities, leading to a diverse array of issue movements outside the labor movement itself. More generally, Ralph Miliband identifies a permanent and growing pressure from below—a state of desubordination—that undercuts governability and evokes demands for a counterrevolution. And Milton Esman interprets the rise of and legitimation of ethnic group claims in Britain's periphery in terms of trends common to post-industrial societies.

Economic decline and loss of world power have coincided with the emergence of a fragmented, multiracial populace. Increasingly, Britain is threatened by the proliferation of autonomous and incompatible systems of power and values which work not only against the center but also one another. George Orwell's characterization of England as "rather stuffy Victorian family" is surely no longer accurate. While Kramnik's volume makes a useful addition, the real need is for a fuller analysis of the fundamental changes in the operation of the external and internal systems that are producing the disintegration of the British nation-state. Only then will it be possible to determine whether the British marasmus provides a model for other late industrial societies.

JOEL D. WOLFE

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Local Government in Britain and France: Problems and Prospects. Edited by Jacques Lagroye and Vincent Wright. (Boston: Allen and Unwin, Inc., 1979. Pp. viii + 244. $19.95.)

The Reorganization of British Local Government: Old Orthodoxies and a Political Perspective. By John Dearlove. (New York: Cambridge University Press, 1979. Pp. viii + 308. $34.50, cloth; $8.95, paper.)

Jacques Lagroye's and Vincent Wright's collection of essays on local government in Britain and France upholds an unfortunate tradition in comparative urban politics. The reader must accept a series of *"entre nous"* assertions, disembodied institutional histories, and considerable repetition in order to extract the authors' numerous contributions to this literature. John Dearlove's work on British local government reorganization is largely devoted to a critique of the academic tendencies that lead to these shortcomings. So important are the "orthodoxies" that Dearlove attacks that his own book is somewhat the captive of their rigidities. Together the two volumes demonstrate the persistent empirical weakness of the comparative urban politics literature.

Lagroye and Wright assembled for their book 13 essays by experienced and knowledgeable scholars of French and British local government. The papers with varying degrees of success introduce the reader to major issues in territorial politics, recent ventures in institutional reform, local political sociology, and municipal finance for the two countries. In general, the essays are paired—one on each topic per country.

Dearlove's book, which is *not* about the 1974 reorganization of British local government, simultaneously explains why the Lagroye-Wright papers fall short of ideal political comparison and then falls victim to the very institutional trap he criticizes. Dearlove's book consists of a loosely connected set of essays about local democracy, administrative efficiency, and class domination. The connecting theme is that of "old orthodoxies": those who have thought about or acted on local government reorganization have either held naive assumptions about local democracy and centralize efficiency or *realpolitik* conclusions about the likelihood of working-class control in urban areas. Dearlove's arguments on these points will be familiar to those who have read the pluralist-anti-pluralist debates of the late sixties, Harold Seidman and Aaron Wildavsky's work on organizational and managerial reform and the structuralist-instrumentalist debate among Marxist (especially French) political economists.

Dearlove's contribution is to bring these arguments to bear upon the late 1960s and early 1970s British debate about local government reform. He shows that in Britain, as occurred somewhat earlier in the United States, reform contained a heavy component of "businesslike efficiency" and middle-class or business bias. In making this connection, Dearlove argues that reorganization in British local government as in other cases may nudge administrative practices or marginally shift relative access, but it will not greatly alter the ability of analogous institutions to deal with ongoing political, economic, social problems. The naive and conservation orthodoxies about local government prevent, however, a more serious and critical analysis of territorial political issues.

The Lagroye-Wright papers indeed illustrate the constraining effects of these orthodoxies on the study of British and French local government. The authors, tightly bound by the institutional category local government units provide, direct their inquiries towards the development and difficulties of these given units. Consequently, author after author dwells upon central government reports and proposals for regional, administrative, and financial reform without revealing very much about how, in fact, the institutions operate before or after reform. Instead, we have a highly centralist view of territorial politics in Britain and France that visualizes a central government and an undifferentiated network of local governments, the former struggling to implement reforms, the latter struggling to resist all but those that would produce more independent finance. In their introduction, the editors recognize that within-country differences may be greater than those across countries, but their authors almost systematically fail to pick up this theme.

This result is not surprising. Because students of British and French urban politics have allowed institutions to identify analytic issues, they are drawn to organizational matters and away from issues that cut across institutional forms. Consequently, the authors have little empirical material with which to illustrate the ways in which British and French local authorities deal with urban political, social and economic matters. All French scholars, for example, deal with central-local relations but must rely upon studies a decade and a half old to substantiate their arguments. Accepting institutional orthodoxies, these authors have also been content to follow "failed" reform and ignore the actual operation of urban governments. If the American study of urban policy is or was overwhelmed with disjointed case studies, the British and French literature is overwhelmed with attempts to synthesize in the absence of a sound empirical basis.

This institutional framework and the volume's own organization produces the book's most lamentable characteristic: that the authors learn so little from one another. The book is not, with a few exceptions, a *comparative* study. Only L. J. Sharpe's essay "Modernizing the Localities" makes a systematic effort to compare the phenomenon in question across the two countries. Failing detailed comparisons on the specific topics like institutional reform or municipal finance, the editors cannot reach even their modest goal of comparison, "To know not only what a system is, but also what it is not, and why not" (p. 1). Instead, the authors pursue their topics largely oblivious to questions suggested by contrasts with the other system.

This tendency to overgeneralize and underobserve can, in part, be attributed to Dearlove's old orthodoxies and their implications for organizational tinkering and top-down reform. Unfortunately, Dearlove himself is unable to leap from either the orthodox or the Marxist perspectives he presents to a more satisfactory approach to the study of urban politics. By the end of his book (pp. 244–45) Dearlove is reluctantly trying to decide among interpretations of local governments as autonomous, as parts of central government, or as tools of business even though he has spent pages telling us why the institution must be superseded, at least analytically. He can recognize (pp. 238–39) that local government in Britain is not and will not be equal to modern socioeconomic crises which occur in urban areas, but his own institutional trap will not permit him to see the consequences for scholarship. Local governments must be seen as nodes on a more complex socioeconomic political network interesting as a group of actors that affect the way the network operates, but not as the fixed starting point for scholarship. We are on firmer ground beginning with urban issues—policy, participation, class conflict—and continuing by asking for the institutions that deal with those problems, rather approaching the study of urban politics from the other way around.

Dearlove's annoying habit of building his sentences with quotations taken from other sources constrains his argument. One wonders if he would have pushed his analysis further if he had relied more on his own writing. The fortunate consequence of this stylistic device, however, is a 42-page bibliography emphasizing British urban politics.

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The Breakdown of Democratic Regimes. Edited by Juan J. Linz and Alfred Stepan. (Baltimore and London: Johns Hopkins University Press, 1979. Pp. 718. $35.00, cloth; 4 vols. paper, $2.95 and $3.95 each.)

Take one very significant and understudied topic; place it in the hands of two illustrious editors; select an international panel of 13 distinguished scholars to write essays covering 11 countries on two continents; allow them 15 figures, 82 tables, 718 pages and 1185 footnotes for ingredients; let simmer, stirring occasionally, for nine years; then publish the result simultaneously in one sizeable hardbound or in four paperback volumes—and you have the recipe for *The Breakdown of Democratic Regimes*. Tantalizing, definitely; appetizing, probably; digestible, possibly; repeatable, doubtfully; reviewable, implausibly.

One is tempted to evaluate this mammoth undertaking in the style of the *Guide Michelin* (the red one, that is) allotting varying numbers of "stars" for, say, the content of intellectual perspicacity and "knives and forks" for, say, the style of scholarly argumentation, but withholding judgment on the total effort. The editors in their brief preface encourage this evaluation by explicitly denying any unity in design or product—any attempt on their part "to force individual contributors into the procrustean bed of [their] own thinking." They call the reader's attention rather gleefully to "important differences in the authors' intellectual orientations which grew in part out of the diversity of democracies studied and reflect in part genuine differences of opinion on the relative weight to be attached to political forces." Despite these entreaties, I will make an effort to treat *Breakdown* as if it were a stew, rather than a *smörgåsbord*—if only because I doubt whether the typographer could furnish me with those cute little Michelin symbols I would otherwise need.

Juan Linz's leadoff essay on "Crises, Breakdowns and Reequilibration"—itself a separate volume in the paperbound version—sets a high standard of inquiry for the entire collective effort. It is comprehensive in scope, erudite in learning, thorough in documentation and wise in its maxims and conclusions. It is also prolix in length, illustrative in argumentation, digressive in focus and conservative in its values and implications. As an effort in intellectual leadership (notwithstanding the denial in the preface), it unquestionably succeeded in inciting the other contributors to a high level of scholarly craftsmanship, to an attention to longer time frames for explanation and to a general need to focus in detail on political factors in explaining such a political outcome as regime breakdown. This alone entitles the book to a place of honor on the shelf of systematic, multi-authored comparative *monumentae politicae* of our times—along with such equivalents as *Mass Politics* by Allardt and Rokkan, *Party Systems and Voter Alignments* by Lipset and Rokkan, *Political Opposition in Western Democracies* by Dahl, *Authoritarian Politics in Modern Society* by Huntington and Moore, and the innumerable products of the SSRC's Committee on Comparative Politics.

But if these neighbors-on-the-shelf are any indication, *Breakdown* would mark the end rather than the beginning of a line of inquiry—a learned resumé of the state of past theory and research, rather than a speculative assertion of the direction they might be expected to take in the future. *Breakdown* as a whole might escape that archival fate, if only because of the unexplored novelty of its subject matter and the relative youth of its contributors; however, certain qualities of Linz's introductory essay lead me to suspect that it will be more cited than read, more used in parts than exploited as a whole.

First, Linz's approach to the admittedly complex topic of the breakdown of democratic regimes is highly eclectic and broadly inductive. He eschews the two strategies of theory-building that might have produced a more focused and tightly reasoned object for subsequent empirical testing or critical reaction. He neither explores deductively the generic properties of democratic regimes and motives of political actors which might result in some predictable trend(s) toward dilemmas and contradictions between performance and expectations, and eventually to breakdown, nor does he explicate inductively the specific properties of regime challenge, consolidation and/or collapse in a single historically identifiable case or cases which might produce a "model" or "models" against which other cases might be compared and contrasted. This latter approach Linz used to great effectiveness in his essay on "An Authoritarian Regime: Spain" (in E. Allardt and S. Rokkan [eds.], *Mass Politics: Studies in Political Sociology,* Free Press, 1970, pp. 351–83) which became of seminal importance to a large number of scholars working on a widely dispersed set of countries. His *Breakdown* essay lacks this consistency. One is treated to insightful observations and encyclopedic comments on many cases (and even to a few "non-cases" in which democratic regimes persisted in the face of serious challenges) without a clearly demarcated core sequence of interrelated processes and events. The fact that not one of the country authors makes an effort to apply the *Gestalt* of Linz's essay to the specific case confronting him perhaps illustrates the problem. At best, they pick up an observation here or a concept there, and discuss its relevance. Granted the egocentric nature of scholarship—especially in such a lively and innovative group as was assembled for this project—and granted the problems in sequencing individual contributions to a collaborative volume which make the systematic exploitation of introductory orienting frameworks difficult, if not impossible, nevertheless, I suspect that here the absence of either a deductive, abstract model or an inductive concrete case study as the basis for theory building will impede its assimilation by interested scholars. Certainly anyone who approaches Linz's essay with the idea of obtaining a "quick fix" for the analysis of a given instance of democratic breakdown or for the testing of discrete hypotheses about political change is bound to be disappointed.

Second, the conceptualization Linz brings to the topic is rather loose, under-specified and operationally vague—as is undoubtedly appropriate in such an exploratory essay. It is, however, notoriously difficult to measure accurately and comparatively such properties as "loss of legitimacy," "semi-loyalty to regime," "*Koalitionsfähigkeit,*" "abdication of responsibility," or worse, "abdication of democratic authenticity," "narrowing of the political arena," "leadership"—all of which figure prominently in Linz's scheme. Perhaps when assessed in the mind of a single, well-informed, dispassionate observer these terms can be analytically useful across a fairly wide range of units and periods (Linz is careful to restrict his generalizations by cultural area and epoch), but are multiple analysts of different academic training and political disposition likely to agree on their instance and impact? Hardly.

One category which looms large in Linz's scheme—indeed, which occupies a central position in it—is sure to attract critical attention: "the unsolvable problem." This property is not only suspiciously retroactive, i.e., only after a democracy has collapsed can it be said to have faced an unsolvable problem, and if it doesn't collapse, its problem is thereby revealed as "solvable," but it is also dangerously close to that which it is supposed to cause or predict. Interestingly, perhaps mercifully, none of the case studies relies on this notion. Paolo Farneti concentrates on "the crises of the parliamentary state," rather than any specific substantive problem to explain the rise of Fascism in Italy. Rainer Lepsius does dwell on the economic crisis which undermined the viability of the Weimar Republic—but not as unsolvable—and stresses rather the rise of Hitler's "charismatic community." Walter Simon also mentions "economic distress" in the case of Austria—as affecting but not determining the breakdown—and then relies heavily on the external factor of Hitler's rise in Germany to explain the outcome there. The Alapuro-Allardt essay on Finland does suggest a possible unresolvable problem—"the perception of a Communist threat and its elimination"—but that was a case of near-miss rather than collapse. Nothing in Peter Smith's account of Argentina (1916–1930), Alfred Stepan's of the Brazilian coup of 1964, Julio Cotler's of the Peruvian breakdown in 1968 or Alexander Wilde's analysis of the collapse in 1949 and reestablishment in 1958 of democracy in Colombia points to a distinctively "unsolvable problem." One suspects that this particular aspect of the Linz formulation will be rather quickly added to the scrapheap of "unrevealing answers."

The principal conceptual assertion of the essay, one which is likely to persist and is already reflected in the country essays themselves, lies in the notion of a democratic *regime* as distinct from a democratic *government,* on the one hand, and a democratic *society* on the other. "Substantive democracy" and/or the nature of the social and economic order which is protected and promoted by political democracy is ruled out as a focus of concern by Linz on the rather flimsy (and after Chile, in particular, rather implausible) grounds that "[political] democracy can serve many and changing ends, and can defend and contribute to the creation of different social and economic orders." Political actors who argue that existing, formally democratic procedures (Linz's definition of democracy is resolutely Schumpeterian-Lipsetian) perpetuate non-democratic outcomes are *eo ipso* extremists, disloyal or at best semi-loyal opponents of democracy who must be resisted and/or eliminated if that regime form is to survive, according to Linz. (In his essay on Chile, Arturo Valenzuela takes this point to its logical conclusion: "Every effort to maintain democracy is well worth the price" [p. iv]. When Barry Goldwater said this, he, not his opponent, was [rightly] regarded as an extremist.) Inversely, those who attack the occupants of regime institutions on the grounds that, as a government or ruling elite, they are not observing the rules of the democratic game, are also treated as enemies of democracy. Essential to both his analytical definition of the problem of governance and his practical suggestions for governing is the assertion that democratic institutions are (not just "should be") valued and supported irrespective of who benefits from their policies and how those who occupy their roles behave once in office provided certain procedural rules and citizen rights are respected. Nowhere is this empirically proven or critically examined—neither in the introductory essay nor in the subsequent country chapters.

Conversely, one could suggest that democracy is a uniquely contingent form of political domination, dependent on the satisfaction of a pattern of material and ideal interests held by the citizenry and on the conformity of its leaders to certain self-imposed standards of legitimate behavior. It rests neither on a harmony of interest in its survival, nor on a consensus of values inculcated in mass publics. Seen from this perspective, many—if not most—of those who oppose a given democratic regime may be seeking not so much to destroy the regime itself, as to protect it from its corruptors or to extend it beyond its detractors. The obvious fact that "anti-democrats" or proponents of other forms of political domination may seek to capture and are often successful in distorting the aims of these protectors and extendors of democracy does not detract from the point, although it clearly makes it difficult to analyze threats to regime persistence in terms of loyal "good guys" versus semi-loyal and disloyal "bad guys."

The substantive theme of Linz's essay which is most likely to stick in the mind of his readers and to become the subject of future controversy is his assertion that democracies have more to fear from their supporters (and beneficiaries) than from their opponents. This is provocative, even counter-intuitive, since most commonsense notions and existing descriptions stress the overthrowing of such regimes rather than their "overgiving," the seizure of power, not its abdication. The literature on the collapse of Weimar Germany, at least since Karl D. Bracher's magisterial study (*Die Auflösung der Weimarer Republic*, 1964) has long argued this. Linz extends the point imaginatively to cover other cases of democratic breakdown in Europe and Latin America during the interwar and post-World War II periods. The compellingness of this argument is increased by Linz's aforementioned inability or unwillingness to distinguish between opponents of democratic rule as such, advocates of democracy as a means to attain a more democratic social and economic order, and critics of a given democratic government. Since the latter "advocates" and "critics" are among the most likely of the previous supporters and semi-loyal opponents to go over to the other side or to vacillate in moments of crisis, their "desertion" of democracy may indeed be critical to regime breakdown. Inversely, since Linz's "true democrats" are those who presumably value that configuration of institutions regardless of the interests protected and promoted, it should come as no shock to more realistic (and materialistic) students of democracy that, when faced with choosing (to paraphrase Engels) between "the right to democracy and the right to property and privilege," some of them traitorously choose the latter—and in so doing, reinforce Linz's point. Indeed, in those cases where democracy broke down in the context of a marked rise in citizen participation and a real or threatened decline in the prerogatives of property, e.g., Italy in the early 1920s, Spain in the mid-1930s, Brazil in the early 1960s and, above all, Chile in the early 1970s, the encouragement and/or acquiescence of "loyal" republicans, liberals, Christian democrats and even social democrats for a *Machtübergabe* was crucial. Had the country studies been divided not by conventional geographic categories (in the paperback version, the Western European cases are bound in a single volume; the Latin American ones are divided into a multi-country volume and a single lengthy study of the fall of the Allende government by Arturo Valenzuela), but by analytic criteria, I suspect that distinguishing between "aggressive" and "defensive" political contexts might have produced, not a single paradigm, but radically different scenarios of democratic breakdown. (Another analytical strategy that might lead to different conclusions would have been to take advantage of the literature on *types* of democracy. Except for an insightful digression on the differences between parliamentary and presidential arrangements by Linz, neither he nor his collaborators make any use of these distinctions. For all of them, democracy is a single and presumably equivalent regime type.)

Having foregone the use of "stars" and "knives and forks," I will not attempt to evaluate the individual (and, in few cases, collaborative) case studies. They are by no means equivalent in quality, but they do vary within a rather narrow and elevated range. As a renegade Latin Americanist sensitive to the scurrilous allegation that "second-rate areas produce second-rate scholarship," I was pleased to note that the contributions from that part of the world were effectively a match for those from the "old continent." Indeed, in their greater attention to economic and social structural factors and to the impact of external constraints, they tended to go beyond both the introductory paradigm and the more narrowly political expositions of the European authors.

My strongest complaint concerns the absence of a conclusion. Admittedly, the time and effort which went into compiling such a volume must have been enormous, but it is regrettable that no one took the occasion to reflect on the experience of intellectual collaboration, to re-examine its initial presumptions and perspectives in the light of subsequent evidence and/or to formulate an agenda for future inquiry.

My heartiest compliment goes to the editors and the publisher for their imaginative idea of presenting the work in both a monumental and a practical format. It is to be hoped that its availability in multiple, inexpensive paperbound volumes will ensure its use in a wide variety of courses and seminars by comparative generalists and area specialists and therefore *The Breakdown of Democratic Regimes* will not suffer the fate of its predecessors—permanent consignment to the dusty realm of past scholarship, or worse, to the ignoble status of a hefty bookend.

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Marxism-Leninism in the German Democratic Republic: The Socialist Unity Party (SED). By Martin McCauley. (New York: Barnes & Noble, 1979. Pp. xix + 267. $23.50.)

This book surveys political developments in the German Democratic Republic (DDR) from the establishment of the Soviet Occupation Zone (SBZ) in 1945 through the first half-decade of the "Honecker Era," beginning in the early 1970s after the political demise of Walter Ulbright. The title may be somewhat misleading, since this is not primarily a book about Marxist-Leninist ideology, but rather a political history of East Germany. To be sure, since the central role in this history has been played by a political party claiming to be Marxist-Leninist, it is not surprising that matters related to that party's ideology constitutes an important theme. Yet Marxism-Leninism is not a dominant or even organizing theme, as the title of the work, some of its chapter headings, and the author's stated main thesis suggest.

The study explores a wide range of historical events and other factors that have influenced political developments in the DDR over the years. McCauley shows, for example, how significantly the DDR has been affected by the changing state of East-West relations, and how, repeatedly, the Soviet Union has allowed its Great Power interests to take precedence over the interests of its Marxist-Leninist sister party. Indeed, at least until 1955, when the USSR established diplomatic relations with the DDR, the SED leadership had to live with the possibility that the Soviets would reach agreement over their heads with the Western allies, and barter away the DDR in exchange for concessions elsewhere (p. 87). We are also shown how strongly the policies of the SED leadership were motivated by their obsession with international recognition for the DDR and ambitions to outshine West Germany. We see how much the personal ambition, political aims, leadership style, and tactical skills of Walter Ulbricht influenced the character of the East German state.

Of course, these and most other matters discussed in McCauley's book have ideological ramifications, even where ideological considerations cannot be considered important causal or constraining factors. Since Marxist-Leninist parties base their right to rule on the claim that they are guided by a truly scientific ideology, even decisions reached on nonideological grounds must be legitimated in ideological terms. It is thus interesting to inquire into how the SED, whose ideological foundations have been subjected to exceptionally severe stresses, has striven to maintain ideological integrity.

Something like this appears, in fact, to be McCauley's intended focus. In the preface he asserts that in 1945 "The question was not whether socialism would prove victorious, but what face socialism would show to the world—German or Soviet? This study argues that if the working class in the Soviet Zone had carried the day, German socialism would have been born" (p. ix). Unfortunately, not only is this thesis too vague to provide much focus, but also little effort is made to pursue it systematically. At one level it is obvious and hardly worth arguing.

There is no question that any of the East European Communist parties would have of their own choice imposed the Soviet model as rigidly as Moscow required after 1948. Beyond this, despite intriguing chapter titles like "The German Road to Socialism" and "Socialism with a German Face," the author does not make sufficiently clear the precise nature of the conflicting faces of socialism, let alone pursue systematically the perennial tensions between them.

Apart from this lack of focus and of thematic unity, McCauley's book provides a concise, informative and readable overview of history, politics, and government in the DDR. It would serve as a good introduction, not only to the DDR, but also, more generally, to the Soviet-type regimes of Eastern Europe. The author has successfully avoided the unidimensional character of so many chronologically organized country studies. Partly by breaking his historical account into five "eras," McCauley skillfully blends in substantial horizontal digressions into areas of inquiry such as the membership structure of political parties in the SBZ, the institutional structure of the SED, the nature of a "party of the new type," the economic base of the East German state, Soviet policy toward Germany, the "New Economic System of Planning and Management of the Economy," the "Scientific-Technical Revolution," etc. Such digressions not only provide a sense of the contexts of political events and issues; they also contribute to a richer and fuller picture of the kind of state and society the DDR has come to be.

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Governmental Instability in Indian States: West Bengal, Bihar, Uttar Pradesh and Punjab. By Subrata Kumar Mitra. (Delhi: Adjanta Publications, 1978. Pp. 150. Rs. 40.)

This is a craftsman-like study the purpose of which is "to develop a formal model that explains government stability, while having some applicability to the problem of constitutional stability as well" (p. 1). The analysis does not attend to the latter, but focuses exclusively upon the former concern in the case of four Indian states, all of which experienced a congeries of governments after the general elections of 1967. The cases are interesting ones, and they are appropriate. The concern is simple and important; the method appropriate though complex. That which is to be explained, government stability, is defined in terms of its measure, i.e., (1) the extent to which a government lasts its maximum possible term; (2) the incidence of ministerial resignations; and (3) the extent to which President's Rule is imposed. Two principle variables are used to explicate, i.e., (1) the affinity of potential coalition partners in terms of ideological space, and (2) the incidence of fractionalization of government and assembly.

The strength of this study rests in the systematic development in chapters 2 and 3 of the model to be employed—a model of coalition behavior using the theory of *n*-person cooperative games. The concepts are not new, though their use involves certain elaborations and extensions of previous work. The formulation of ideological space draws upon such work as that of Axelrod (1970), Niemi (*APSR*, 1969) and the thought underpinning Riker and Ordeshook (1973), although the latter's assumption of monetary payoffs is replaced by an assumption of "a favourable outcome in an important public policy area as the payoff to the winner" (p. 27). The application of fractionalization—the probability that any two randomly drawn units from a partitioned population will be from the same partition—derives from the formulation of Taylor and Herman (*APSR*, 1971). The perusal of this literature together with its adaptation to data available for the Indian case are thorough and tightly drawn, and constitute the most useful part of the book; indeed, they merit further encounters with data.

The major difficulties of this book, and they are major, concern the specification of the explicandum and the meaning of data used to define ideological space. It is not at all certain, for example, why ministerial resignation is indicative of cabinet instability unless it is so by virtue of assertion. Resignations in principle and in reality may reflect a strengthening of a cabinet—trimming it of fat (augmentation of side-payments, be they material or ideational, for partners left) or bolstering support (drop the weaker for the stronger or the less affinal for a partner more so). A ministerial resignation may be an indication of strength and cabinet stability. The meaning of a resignation must be established before it is coded one way or the other. The reallocation of portfolios, of course, is another candidate as a measure of instability, but subject to the caveat above.

A second problem has to do with the specification of ideological or issue affinity. Mitra notes that votes in the state assemblies would be a more reliable indicator of ideological and issue preference and position than the condensation of party platforms—the latter being the sources of data used in the present study. With this we agree. A content analysis of speeches in the assemblies would also be a more appropriate candidate, though a much more time-consuming one. Either would be more reliable than election manifestos which are notoriously compromised documents. Furthermore, relying exclusively on party platforms makes it impossible to determine ideological and issue distance among factions within the Congress party. Indeed, factional coalitions within Congress governments—many of which were the most stable state governments in India—if judged in terms of ideological space may deliver profiles and scores not unlike those of multi-party coalitions. If this be true, then there is something more than ideological distance that must be found to explain stability or instability.

The above also raises the question of what it is that partners in government agree upon and/or fight about. The assumption in this study, as noted above, is that they contend over public policies, and this is often true. No less frequently in the Indian case, however, the most contentious conflicts concern calculations of material benefit and the development of more advantageous positions for future games. Different definitions of preferred outcomes may not only vary among partners of a given coalition, but the distribution may also vary with time. In his investigation of coalition formation in the same states as studied in this volume, as well as others, Brass suggests (*APSR*, 1968 and 1977), and quite persuasively so, that calculations of future advantage and considerations of local power were determinative in the scramble of cabinets. Bueno de Mesquita (1975, *APSR*, 1974) finds need achievement of party leaders a critical variable in accounting for coalition behavior and performance. The current study does not address alternative explanations such as these.

Despite these reservations, this study deserves the perusal of those interested in formal models of coalition behavior, and if it had addressed a richer data set and had explored it at greater length, it would be of considerable interest to a wider body of comparativists. The Indian experience constitutes a veritable laboratory for inquiries such as this. But there is much yet to be learned, and much more to be done. The doing is to be encouraged.

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Marcos and Martial Law in the Philippines. Edited by David A. Rosenberg. (Ithaca, N.Y. and London: Cornell University Press, 1979. Pp. 315. $17.50.)

Marcos and Martial Law in the Philippines "examines the decline of constitutional democracy and the rise of authoritarian government in the Philippines" (p. 9). Unlike many edited volumes from conference panels, the chapters are well organized around the theme of martial law, and the contributors—two Filipinos and three Americans, all political scientists—are well-respected Philippine specialists with extensive field research experience in the country. In addition to a useful introduction presenting the various pro- and anti-government arguments for the declaration of martial law, Rosenberg includes 14 appendices containing various documents on the New Society.

In a separate chapter, Rosenberg analyzes the seizure of the mass media and their use to foster the programs and goals of the government. Marcos justified his actions because of alleged communist influence in the industry, oligarchic control of the press, radio, and television, and the need to use the media for social change. Although some of the pre-martial law flavor has returned to the press in recent months, journalists remain wary of Marcos' repeated claims that the press is free. Many were detained at the beginning of martial law, others have been intimidated since, and foreign correspondents have been harassed by the regime. Clearly, no criticism appears in the media of the president, the first lady, or of key government officials.

Attempting to link ideology with the accomplishments of the New Society, Jose Abueva gives the regime high marks in few areas. The only clear A-plus is in the area of tax collection. Yet promises of increased social discipline and value changes leading to greater social and economic equality and less oligarchic political control are hard to discern. What is obvious are the heightened profiles of the military, cabinet ministers, government technocrats, and persons close to the president and the first lady. This is in sharp contrast to the worsening economic conditions of the poor. According to Abueva, because of unfulfilled promises, suppression of dissent, and political manipulation, the government lacks legitimacy, and now Marcos faces the "reformer's dilemma": how to continue reforms without disturbing the coalition that supports the New Society.

Rolando del Carmen discusses the constitutionality of martial law and developments within the judiciary since 1972. To be sure, Marcos had the constitutional authority to declare martial law, but the use of citizens' assemblies to ratify the new constitution was questionable. Previously a coequal branch of government, the judiciary is now subservient to the legislative branch, and under martial rule, all judges sit at the pleasure of the president. Moreover, decrees, orders, and acts issued by Marcos under martial law are not subject to judicial review, and many judicial functions have been taken over by military tribunals. Thus del Carmen maintains that while constitutionalism "is by no means dead," it is "pale and anemic" in the Philippines today (p. 109).

The success or failure of martial law, according to Marcos, should be judged by the accomplishments of the land reform program. In a carefully researched chapter, Benedict Kirkvliet argues: (1) that the real purpose of land reform is to quell rural insurgency rather than redistribute the wealth; (2) that landlords are able to protect their interests by manipulating the political system to resist reform; and (3) that, mainly as a result of the first two reasons, the extent of the land reform program is restricted. Kirkvliet places the present program in an historical context and shows how the government uses it to gain legitimacy, while making few real concessions to the peasantry. Similarly, he discusses the various ways the program is subverted and circumvented by the landlords, and stresses the fact that the program excludes farms of less than seven hectars and land planted in crops other than rice and corn. Also underscored are the many financial obstacles to the peasant's purchasing the land in the allotted time. Finally, Kirkvliet compares the Philippine program with land reform elsewhere, and concludes that land reform in the Philippines is not radical, for it only benefits the peasants slightly.

Applying theoretical models developed by Johan Galtung to the Philippines, Robert Stauffer presents a forceful and intriguing argument that since the declaration of martial law, a "refeudalization" process has taken place. Stated simply, refeudalization here refers to the reversal of the nationalist trend in Philippine politics in the late 1960s and early 1970s to greater dependence on the United States and the larger world system. In support of the argument, Stauffer analyzes a number of pre- and post-martial law conditions favoring the growth and maintenance of the refeudalization/authoritarian model in the Philippines. He documents how Westernized technocratic, business, and military elites (all with strong ties to America) have increased in power and influence since 1972; how the regime's development programs are heavily indebted to international lending institutions; and how key sectors of the economy are dominated by foreign interests. He also discusses the curtailment of autonomous political institutions and activity and the suppression of dissent in an attempt to create support for the present government. In the final analysis, however, the regime's increasing dependency is much more to the advantage of the U.S. and international interests than to the Philippines—an effect just the opposite of the stated goals of the New Society.

While the Philippine specialist may have preferred additional chapters on the Muslim and Communist problems in lieu of the appendices, this is an important book on martial law. The chapters are clearly written, well documented, and carefully argued. Those concerned with the Philippines and U.S. policy in Southeast Asia will find this a provocative (and perhaps troubling) book.

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Chile: The State and Revolution. By Ian Roxborough, Phil O'Brien and Jackie Roddick. (New York: Holmes & Meier, 1977. Pp. x + 304. $20.00.)

The Overthrow of Allende and the Politics of Chile, 1946–1976. By Paul E. Sigmund. (Pittsburgh: University of Pittsburgh Press, 1977. Pp. xiii + 326. $14.95.)

The two books under review present markedly contrasting evaluations of Chilean politics and society prior to the 1973 military coup because they both address very different questions. The Roxborough et al. book attempts to explain why revolution failed in Chile, while the Sigmund book seeks to explain why Salvador Allende was overthrown, bringing to an end decades of democratic rule.

Roxborough, and his colleagues from the University of Glasgow, paint a picture of an underdeveloped dependent society in which the working class is gradually able to assert itself through a limited and distorted democratic process. The increasing radicalization of the working class and the divisions of the bourgeoisie which come to the fore during the government of Eduardo Frei (1964–70) set the stage for the election of Salvador Allende in 1970 and a unique opportunity to begin a revolutionary process leading to a socialist society. The Chilean road to socialism failed in large measure because of the virulent opposition of the right and its allies in the United States. But this failure would not have been inevitable had the Popular Unity government been able to chart a more revolutionary course. Instead, Allende and his colleagues, by continually seeking to build bridges to moderates in the opposition to maintain the constitutional process failed to realize that "Chile had entered a process of radicalization in which the politics of rhetoric and limited reform would no longer work" (p. 166). As a result, the government itself "demoralized key sectors of the vanguard and confused many more of the working class" (p. 163), making revolution impossible and a coup unavoidable.

This simple and compelling thesis has two fundamental flaws. In the first place, it assumes that the working class had indeed achieved a high degree of revolutionary consciousness. But the authors provide no concrete evidence of widespread support for a revolutionary posture. In describing the Frei administration they imply that government policies had the effect of radicalizing the working class and increasing the support for leftist parties. Though they report the percentages of the votes received by the candidates in the 1964 and 1970 presidential election, they fail to mention the fact that Allende won in 1970 with a smaller percentage of the vote than he received in 1964. Their only evidence for heightened revolutionary consciousness during the Allende period was the "programme" of the Cordon Cerrillo's (p. 171). There is no attempt to estimate the number of workers who participated in the Cordones or to judge the extent of support which the revolutionary posture had among workers. It is not at all clear that Chilean workers would have given unqualified support in massive numbers to a policy of radicalizing the political process.

In the second place, the argument assumes that had the government moved to provide revolutionary direction, the opposition and the military would have been unable to respond. Indeed, the coup did not take place earlier precisely because of the effort to preserve the constitutional process which kept the military neutral. The coup became more and more likely not because of the actions of the government, but because of the actions of the revolutionary groups which made it increasingly more difficult to build a center consensus to preserve the regime. In that sense those supporting a "people's power" were engaged in a self-fulfilling prophecy. It certainly is true that adherence to constitutionalism made a revolution impossible, but given the alternatives of an authoritarian regime, "limited reform" looks very good today.

Unfortunately, though the book is well written, and presents its principal thesis in a clear fashion, it leaves much to be desired as a contribution to scholarship. The authors have very few footnotes (chapter 10 on the military government only has one with four others apparently missing). Direct quotes are often not cited, and the many references to party documents, political events, statistics, and historical developments have no documentation whatsoever. Large portions of the historical section seem to rely on a handful of sources and are replete with errors.

From a scholarly point of view, the Sigmund book is much more valuable and is a permanently significant contribution to the debate on the Allende years. Restricting himself to the much shorter time-frame of the Frei and Allende administration, Sigmund recounts in considerable detail the highlights of both administrations and documents his material very carefully. Unlike Roxborough et al. who organized their book analytically, and provide a simple central thesis, Sigmund follows a chronological approach. Unfortunately, this means that the reader often loses sight of any organizing thread. The book consists of numerous paragraphs with separate subheadings, some to critical events (such as the constitutional deadlock over the areas of the economy bill or the employers' strike), and others to less important ones (such as the report of alleged electoral fraud charged by a faculty member of the Catholic University). The reader is thus deluged by facts of different complexion and magnitude which are not tied to any analytical framework capable of interpreting them. This lack of a central thesis or theses makes it difficult for the author to pass judgment on whether "systemic," "national" or "personal" factors were more important in causing regime breakdown, as he promises to do in the introduction.

Because Sigmund is interested in explaining the overthrow of Allende, rather than the failure of revolution, he concentrates his attention much more on the give and take of the political parties, their strategies and statements, than on broader societal confrontations and forces. In the process we lose sight of the dynamics of the polarization process and the severity of the confrontation between contending forces, and the reaction of the right.

Though Sigmund makes an effort to present a balanced account, and for the most part succeeds, the book is excessively preoccupied with the Christian Democrats. More attention is paid to that party, its internal divisions, political positions, and party congresses, than to any other party. Even though he does at times fault them for their rigidity, Sigmund's interpretations and judgments are similar to those of the right of the Christian Democratic party and are biased in favor of their actions. He thus tends to underplay the extent to which the Christian Democrats contributed to eroding political institutions and procedures during their years in office. He also minimizes the Christian Democrats' inability to perceive that the political game had changed after June of 1973 and that they should have made every effort to save the regime.

The Sigmund book is marred by a few minor errors. The most important is his failure to recognize that the erosion of electoral support for Frei was not due to the "fickleness of Chilean voters" (p. 19) or the "perenially dissatisfied voter" disappointed with the drop in personal income (which did not take place) (p. 110), but to the stability of electoral allegiances. After the presidential election, rightist voters (encouraged to vote for Frei in 1964) simply returned to their own parties. The failure of the Christian Democrats to realize that the vote for Frei was not an unqualified endorsement of that party contributed in large measure to the arrogant style of rule which helped aggravate the political climate before the 1970 election. Other errors include the following: mayors were not appointed by the president (except in three cities) but elected by municipal councils (p. 16); the general turned political leader and twice president of the country was not Pedro Ibanez but Carlos Ibanez (p. 24); the Ministry of the Interior did not deal with all issues of domestic policy (p. 188)—in fact, the ministry had very few substantive responsibilities, its importance being derived by its jurisdiction over the police and the symbolic position of the minister as vice-president in the absence of the head of state; much more than one-third of the population was in the market economy in the mid-1950s (p. 19)—indeed, Sigmund himself notes that in 1960 35 percent of the population was middle and upper class and 70 percent of the population lived in urban areas (p. 21).

ARTURO VALENZUELA

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The Arab Press: News Media and Political Process in the Arab World. By William A. Rugh. (Syracuse, N.Y.: Syracuse University Press, 1979. Pp. xviii + 205. $18.00, cloth; $5.95, paper.)

The communications system in the Arab world is one of the more underdeveloped fields of academic inquiry in the United States. While it is true that some scholarly essays that dealt with the use of the mass media by national authorities to promote the adoption of a particular ideology, social policy, international alignment, or the pattern of communication flow, have appeared infrequently in one or another of the social science periodicals, the only monograph-length study of any aspect of the media in the Arab world appeared 27 years ago. Tom McFadden's *Daily Journalism in the Arab World* (1953) covered newspapers in five Arab countries as they were struggling to shape their political and social systems. Viewed from this perspective, the present study is a welcome addition.

The Arab Press accomplishes two tasks: on the one hand it describes, in uneven detail, the evolution of the Arab press in 18 Arab states—the Arab press of Palestine and Israel is not included—identifies the major newspapers and their affiliation and the general attitude they display towards authority. On the other hand, it tries to provide some kind of schema by which the newspapers could be grouped and classified. Thus the author is able to classify newspapers by phases of evolution and type—hence the four major phases and types: colonial, factional, mobilizational/loyalist and diverse. The general scheme which the author devises tries to relate each category to the type of political system which prevails in the Arab world. Thus the papers in Egypt (prior to Sadat), Algeria, Iraq, South Yemen and Syria are mobilizational, whereas those of Morocco, Jordan and Saudi Arabia are loyalist. On the other hand, those of Kuwait and Lebanon fit the diverse model. Additionally, the author provides information on ownership, circulation and general coverage of issues. The bulk of the book deals with newspapers; more brief is the discussion pertaining to sources of news as well as the electronic media. On the basis of this latter information one learns that the Arab papers have access to their own news agencies as well as the major international news sources (AP, UPI, Tass, Reuters, etc.); one is informed that, unlike newspapers, the electronic media are state monopolies in almost all the Arab states.

This kind of information is both useful and important; but it is not enough. One would want to know what difference does it make, in terms of actual coverage of issues and attitude towards issues, for one paper controlled entirely by the government and another to be independent. And in the context of the Arab systems of today, does it really make a difference for newspapers whether they are published in Morocco or Iraq?

Answers to such questions could have been obtained by more rigorous techniques, perhaps content analysis. And in the light of the current campaign of the Third World to control the flow of news to their own countries and thus reduce the hegemony of Western information, it would have been helpful to know why and how frequently Arab editors fill their newspapers with stories and issues that are fed to them by the same Western news agencies, much to the chagrin of their governments. Admittedly, the issues are complex but are too important to ignore in a book which deals with the Arab press.

IBRAHIM ABU-LUGHOD

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Local Politics and Nation-States: Case Studies in Politics and Policy. By Ann Schulz. (Santa Barbara, Calif.: Clio Books, 1979. Pp. xii + 234. $21.50.)

The comparative study of local politics suffers from many difficulties, not the least of which is the lack of a framework for analysis or even a commonly accepted set of analytical categories which can be employed to integrate the extensive case study literature. Ann Schulz draws upon that great body of disparate materials in order to develop a series of arguments about discrete aspects of relations between national regimes and their localities. What emerges from this major undertaking, unfortunately, is more pastiche than systematic analysis.

This is not to suggest that Schulz ignores important issues. On the contrary, her survey of the literature touches on such matters as: the nature of local autonomy in different regimes, particularly as such autonomy reflects upon the security interests of national elites; the variety of power relationships which connect national and local elites; the way party systems are structured to tie local politicians to national regimes; relations between and among national bureaucrats and local politicians; the local consequences of interactions among national political and economic interests; and the distributive and redistributive effects of local political systems vis-à-vis national regimes. In her treatments of these significant topics, Schulz has some arresting points to make. Yet one comes away from the individual chapters and from the volume as a whole unsure of the general conclusions the author might wish us to reach.

Because the great variety of case materials that Schulz employs makes the need for an analytical framework particularly apparent, the introductory and concluding chapters are especially disappointing. Both are brief and do not pretend either to prepare the reader for what is to come or to summarize what has gone before. Instead, we are plunged rather too quickly into chapter 2 where we are confronted with a series of generalizations about national-local power relationships in particular political systems. These are the kinds of statements that are all too frequent in the literature of comparative politics—the non-comparative "comparison": "In country X, the situation is A, but in country Y, it is B." Schulz does attempt to overcome this scatter-shot approach, in part, by introducing two fairly detailed case studies into each of the five substantive chapters. Thus in reviewing the linkage roles of political parties and politicians, she follows a wide-ranging survey of the general literature with case materials that focus on Japan and India. Similarly, after discussing local economic interventions by members of the political and economic elite in a variety of systems, she examines data from France and Iran. No doubt, an argument could be made for the selection of specific regimes for closer study, but Schulz makes no such effort. Rather, the longer case studies, much like the selective bits of evidence brought together on particular issues, simply appear to be examples—as good as any other examples—for representing how a variety of regimes deal with certain matters.

Despite the prodigious effort involved in reviewing as much literature as Schulz has done, specialists are likely to disagree with certain emphases or interpretations. Thus, in connection with making an argument that "redistribution has been a prerequisite for centralization and state-building" (p. 153), rather than the reverse, Schulz becomes entangled in a brief discussion of redistribution in the United States which seems to muddle the distinction between redistribution and distribution (pp. 160–61). Equally important, she asserts rather flatly in the case of the United States that "public schools have significant redistributive potential" (p. 165), despite a debate over the last 15 years among students of educational policies which at least has proposed a strong argument to the contrary.

In sum, Schulz's book does not so much advance the field of comparative local politics as it reflects its present character—a growing body of disparate case studies which reflect distinct intellectual and national system traditions. This literature continues to constitute a major challenge to anyone who seeks to develop a theoretically coherent series of statements about the operations of local politics and governments across nation-states.

DONALD B. ROSENTHAL

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Protest Movements in Two Indian States: A Study of the Gujarat and Bihar Movements. By Ghanshyam Shah. (Delhi, India: Ajanta Publications, 1977. Pp. xi + 171. $8.00.)

Ghanshyam Shah presents a detailed description of protest movements that occurred in the Indian states of Bihar and Gujarat in 1974. The studies of the two movements were apparently conducted separately, and perhaps as an afterthought the author put them together to make a book out of them. The first 60 pages of the book are devoted to the movement in Gujarat, the rest to the movement in Bihar. The author leaves it to readers to compare the two movements and draw their own conclusions regarding their impact on the stability of the two states' political systems and the Indian political system as a whole.

In discussing the Gujarat movement, Ghanshyam Shah considers the role played by the different groups in it, the "different shapes" of the movement at different times, and the issues highlighted by it. The Bihar movement was more ambitious in that it not only focused on specific issues such as inflation and corruption, but also aimed at bringing about a total revolution. The "prerequisites" of such a revolution—an ideology, programs and strategies, personnel, and an organization—are considered in the Bihar study.

The two studies are based on interviews, some survey data, newspaper reports, and the author's own observations. Considering the data at his disposal, the author presents a disappointing analysis of the two movements. The major problem with this analysis is the lack of a sound theoretical framework in light of which the two movements could be understood. In the Bihar study the author is forced to use such a framework, since this movement was based on the concept of total revolution, postulated by Jayaprakash Narayan. However, in this study as in the Gujarat study he gets embroiled in the details of the movement and seldom goes beyond the newspaper-style reporting of the events.

After describing the Gujarat movement, the author arrives at the obvious conclusion that it was essentially an urban middle-class agitation (p. 58). His conclusions on the Bihar movement such as: "Programs of the movement did not concretize the objectives of the movement," and "Students were the vanguard of the movement," (p. 160) are not any more startling either.

Despite serious flaws, this study is not without some merit. The observation that the Bihar movement was a protest movement, not a total revolution (pp. 162–63), is of interest to the students of Indian politics, but it needs more evidence than the author offers. The detailed information in this book on the two movements would also prove of some value to those interested in this subject. An article by John R. Wood ("Extra-Parliamentary Opposition in India: An Analysis of Populist Agitations in Gujarat and Bihar," *Pacific Affairs*, Fall 1975), however, provides a much better understanding of the two movements than does this study.

KUL B. RAI

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Wither the State? Politics and Public Enterprise in Three Countries. By Ira Sharkansky. (Chatham, N.J.: Chatham House Publishers, 1979. Pp. xii + 177. $9.95, cloth; $6.95, paper.)

For all the complaints one hears about the steady encroachment of the modern state on the private sector, the actual line between public and private activities is indistinct and becoming more so. This interesting study explores the grey area at the boundary of the welfare state inhabited by those hybrid institutions which exercise at least quasi-public authority but enjoy significant autonomy from the core government departments. In practice, Ira Sharkansky surveys statutory authorities in Australia; government companies, the Histadrut (Labor Federation), the Jewish Agency, and other public authorities in Israel; and special authorities in the United States. The cases were selected, as the author notes, primarily for convenience, but they provide a wide range of data. Sharkansky believes that the proliferation of such institutions outside the direct lines of state authority poses serious issues of democratic accountability and government performance. Furthermore, he suggests that there is an intellectual crisis in political science because of the profession's myopic preoccupation with the formal institutions of the state.

Sharkansky treats each country in separate chapters, but within a common analytical framework. He describes the form and variety of marginal institutions, assesses their effectiveness, and accounts for nationally specific variations in terms of political culture, government structure, and economic context. He finds, for example, that statutory authorities in Australia provide honest and efficient services, despite the absence of highly developed central government oversight, because of a homogeneous political culture which stresses careerism and legalism and an economy which, apparently, produces sufficient wealth to discourage corruption and underwrite quality. In contrast, the entrepreneurialism of Israeli political culture, an economy under stress, and a triangulated political structure composed of government, the Histadrut, and the Jewish Agency create problems of managerial and political accountability.

Sharkansky is commendably cautious in his conclusions. He offers a few "modest proposals" for improving the administration and control of marginal bodies and issues a spirited call for more research by political scientists. His book is a useful introduction to the comparative analysis of public enterprise. As the author intended, it raises more questions than it resolves.

The explanation for the systematic growth of quasi-public institutions on the fringes of the welfare state is the most interesting but least compelling aspect of the study. Sharkansky assumes that it is the quantity and variety of "public demands" for services which overwhelm the capacities of the formal institutions of the state and lead to the delegation of functions to marginal bodies (pp. 3, 7–8, 150). At the least, this line of reasoning needs to be argued and alternative explanations considered. The kinds of services provided and the "publics" which demand them ought to be specified more carefully. The author suggests several reasons why politicians might find assigning tasks to marginal institutions attractive, i.e., expanding programs without adding permanent state employees to the payroll and avoiding direct responsibility for service failures. The only glue which holds these essentially ad hoc explanations together is a discussion of the political advantages of "ambiguity" (pp. 151–52), but this idea is not developed systematically. The three sets of factors used to account for the structure and process of marginal institutions within each country—political culture, governmental structure, and the economy—are employed in an impressionistic fashion. The treatment of the economic variable is especially weak (p. 123).

Finally, I am skeptical that it is a fascination with the state which has led contemporary political science to ignore marginal institutions. It is much more likely that the dominant behavioral and individualistic perspective of the discipline has diverted attention from institutional analysis of any kind. Furthermore, Sharkansky ignores two recent developments outside the behavioral paradigm which do deal with the problem of state power and its structural relationship to organized private interests—the effort to create a Marxist theory of the capitalist state and the burgeoning literature on corporatism. The former could enrich Sharkansky's explanation of the growth of the welfare state; the latter constitutes one basis from which to explore the changing balance between public and private power.

GARY FREEMAN

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The Politics of Africa: Dependence and Development. Edited by Timothy M. Shaw and Kenneth A. Heard. (New York: Holmes & Meier, 1979. Pp. xii + 400. $23.95, cloth; $14.75, paper.)

This book is a mixed bag of 14 original essays, which together are intended to present "both a review and overview of current theoretical and empirical research on Africa" (p. vii). Each essay was originally presented at the 1973 or 1974 annual meetings of the Canadian Association of African Studies; and the editors, together with seven of the authors, represent Canadian universities. Three additional contributors are drawn from the United Kingdom, two are from the United States, and one each comes from Denmark and the West Indies. The entire volume is part of the Dalhousie African Studies Series.

Focusing on five major themes ("Uneven Development and Class Formation," "Dependent Development," "Political Change and Participation," "Aid to Africa: Interdependence or Dependence?" and "Africa and International Politics"), the collection of country case studies and more general treatments seeks to address some of Africa's most critical problems at the national and international levels. It also examines the continent in relation to what the editors term "a group of theoretical issues" (p. vii) that have been raised by radical European and Latin American scholars in response to the behavioral orientation of American social and political research. This "critical" school of thought originates significantly in the Marxist and neo-Marxist traditions, and has evolved such now-familiar approaches as "dependence theory" and the "center-periphery model" to interpret what is happening politically in the less-developed countries and between them and the industrial states.

These ambitious empirical and theoretical goals represent a tall order for a group of papers originally prepared to stand by themselves as limited-purpose analyses. Predictably, the volume's review is more successful than its overview; that is, the book is more empirically enlightening than theoretically useful.

Parts 1 and 2 include two theoretical papers on class formation and three case studies on the center-periphery dependency relationship that is said to characterize African countries' internal development processes and relations with the industrial world. In chapter 1, Robin Cohen argues that imperially influenced historical writing on Africa has consistently denied or misinterpreted the evolution of socioeconomic classes, and how this phenomenon relates to the metropole-colony linkage. He proposes several means whereby to rectify this lapse, and give full explanatory due to "the incomplete and sporadic [but politically important] character of working class formation. . ." (p. 19).

In the following chapter, Henry Cooperstock seeks to identify an emergent system of social stratification in the welter of ethnic, patrimonial, and sectional differences that unite and divide Africans in the post-colonial period. He describes a political-administrative elite, a bourgeoisie and petite bourgeoisie, and a proletariat which in the Marxist idiom may be transforming themselves from "classes-in-self" to "classes-for-self." These categories coexist with, and may be replacing, patronage and ethnicity as the primary bases of modern socioeconomic and political stratification.

The next three chapters, by Jan Jørgensen, Joel Gregory, and Roger Leys, examine "dependent development" in the contexts of pre-Amin Uganda, Upper Volta, and Lesotho. In the superficial and methodologically suspect manner unfortunately typical of some critical writings, each chapter explains its country's developmental problems with reference to a major "contradiction." For Jorgensen, developmental failures in Milton Obote's Uganda can be explained by the direct and indirect effects of "external demands of the capitalist world system" (p. 44). To Gregory, Upper Volta's migration and related poverty problems are created by the country's location at the periphery of a larger center-periphery relationship linking Europe and West Africa. Lesotho's contradiction, according to Leys, lies in its supply of migrant labor to South Africa and in the "semi-proletarianisation" of this labor under a compromised local elite.

Although all of these analyses make extensive use of available data, their ideologically influenced single-cause interpretations fail to inspire total confidence in the validity of these data's application. One cannot help but wonder, for example, about the hidden agenda of a scholar who prefaces his discussion of Lesotho with the following digression: "Is there a fundamental and irreconcilable conflict between Marxist and bourgeois social science? Given that the fundamental purpose of Marxist analysis is not to describe reality but to change it, Marxist analysis is inherently revolutionary. Theory and analysis is a weapon in the organisational, cultural and military liberation struggle. Bourgeois social science emerges out of that situation which is the subject of analysis. Bourgeois theory and models have, in this context, an ideological function in terms of legitimising the structure of exploitation and oppression. . ." (Leys, p. 99).

Happily, the remaining sections and chapters are relatively free of such ideological biases. Discussions of political change and participation in Sierra Leone, Malaŵi, and Zambia are provided by John Cartwright, Richard Hodder-Williams, and Douglas Anglin. Gerald Helleiner contributes an excellent study of foreign assistance and the possibly declining vulnerability of African states to the purposes of aid donors. Kenji Okuda assesses the motives and effectiveness of Canadian aid in particular.

The book's final section includes several interesting attempts to explore Africa's role in contemporary international politics. David Johns seeks to identify interstate inequalities through a comparative analysis of the diplomatic exchange activity of Organization of African Unity members within the OAU system. James Mayall evaluates the implications for Africa of the recently enlarged European Economic Community. John Clark employs a variety of indicators to evaluate patterns of support for selected African international governmental organizations. And Timothy Shaw sheds light on the organizational actors in African international politics, through an historical analysis which is more imaginative than data-based and perhaps empirically accurate.

Taken as a whole, *The Politics of Africa* provides us with a glimpse of what Canadian and European Africanists are thinking and writing about these days. The quality of the individual chapters is highly variable, as can be expected from what amounts to a conference proceedings.

RODGER YEAGER

West Virginia University

The Democratic Process in a Developing Society. By A. H. Somjee. (New York: St. Martin's Press, 1979. Pp. ix + 168. $19.95.)

This is a remarkable little book that could become a minor classic. It is the most successful attempt thus far to bring together a solid understanding of Western democratic theory and an intimate familiarity with Indian culture and civilization. A. H. Somjee constantly demonstrates his firm grounding in democratic theory, and he links such theory to the works of "Indianists," comparative political scientists and political development analysts.

Western democratic theorists are likely to ignore Somjee's book, on grounds that he is an idealist, and perhaps a naive one at that. Many Westerners are also going to be put off by the fact that Somjee's arguments are (necessarily) convoluted and depend for evaluation on a fairly intricate knowledge of Indian society, including caste and generational change. Furthermore, there are major intrinsic problems with the book, the most significant resulting from the way in which its toughest empirical evidence is drawn from a single town in Gujarat. Nevertheless, Somjee's work was a challenging conceptualization of Indian democracy before the 1980 parliamentary elections; it takes on added significance because the events surrounding those elections square with its rather precise arguments at almost every turn.

Somjee is convinced that a democratic process is now so firmly established in India that "there is no other way to constitute political authority except by means of elections" (p. 139). He calls the 1975–1977 emergency India's "crisis of accountability." After 1977, he says, elections have "ceased to be mere charades in which voters were periodically lined up in support of one's entry into Assembly or Parliament" (p. 134).

Granted the continuing mobilization that is taking place, Somjee suggests that India's next significant crisis—"tougher and more prolonged than the accountability crisis"—will be a crisis of distribution. Somjee agrees with most analysts that India's poorer strata lack "identity, consciousness and political will—the three major elements Marx underlined for effective class conflict" (p. 9). He goes on to what will be a more controversial argument, that "the Marxian concept of class does not help very much in conceptual analysis of the Indian puzzle" (p. 83). Somjee sees traditional social groups as being "incapable of generating effective political pressure to secure [promised results]," but he does not see primordial segments banding together to acquire "the characteristics of class, either in their self-perceived economic injustice or in their antagonism to one another, or indeed in their pursuit of remedial political partisanship" (p. 83).

What Somjee does see is a series of "self-paralysing contradictions which often result in the loss of a sense of political direction" (p. 143). These basic contradictions are in turn traced to the exceedingly strong Indian leadership commitment—"often verging on violence"—to *both* complete civil liberties *and* administrative firmness. Somjee's evidence suggests that the political alternatives represented by Indira Gandhi and her opponents—"of administrative firmness *or* civil liberties, one excluding the other"—are viewed by the Indian populace as insufficient. What is in the process of being "worked out," he argues, is a "delicate balance" in which "firmness in routine administration and in implementation of public policy does not always run counter to the enjoyment of civil liberties" (p. 144).

Somjee still questions "whether certain fundamental aspects in [India's] political society have firmed up enough to ensure a revival, in case of future onslaughts, of her democratic process" (p. 129). To overcome authoritarian threats, he concludes, it is not enough "to periodically throw out corrupt politicians and their political parties." The "next item on the agenda," he says, is "the creation of effective and responsive political parties [that can] make an earnest effort to get down to the basic problems of society" (p. 153).

MARCUS FRANDA

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Class Conflict and Economic Development in Chile, 1958–1873. By Barbara Stallings. (Stanford, Calif.: Stanford University Press, 1978. Pp. xviii + 295. $18.50.)

This book is both a case study of the Chilean political experience from 1958 to 1973 and an effort to elaborate and apply a neo-Marxist "political economy" framework to the analysis of economic policy formulation, implementation, and "development outcomes" in Third

World nations. Taking three principal elements in the analytical framework–social classes/class alliances, the state, and the foreign sector–Barbara Stallings argues that

> although the framework . . . is not a theory of economic development, some general propositions can be derived from it. (1) Social classes will combine into alliances to promote their respective interests. (2) Different class alliances will have different development ideologies. (3) There will be a struggle between the alliances to capture control of the state. (4) Once in control of the state, an alliance will attempt to implement its development ideology through a series of economic policies reflecting that ideology. (5) These economic policies will lead to different outcomes in terms of growth and distribution, i.e., economic development (p. 21).

In subsequent chapters Stallings describes the development models of the presidential coalitions that governed Chile from 1958 to 1973 and, using the framework elaborated in the first chapter, analyzes the political economy of each administration. Stallings' study confirms that (1) the three administrations under consideration did have different development models; (2) this fact was reflected in different economic policies; (3) these differences involved distinctive orientations toward property relations (private versus public), the role of the state in the economy, distributive policies (wage-price policy and employment), and the role of foreign investment. According to Stallings, all of the differences followed more or less directly from the class base and development ideologies associated with the three administrations (p. 179).

A great strength of Stallings' analysis is that after considering the differences among the three administrations predictable from the analytical framework, she then gives attention to similarities among the three presidential coalitions. With respect to "demand management" (government expenditures, remunerations, credit), the study finds "much greater similarities" than would have been predicted on the basis of the framework set out in chapter 1 (p. 195). Stallings finds a pattern of increased demand at the beginning of each presidential administration followed by later cutbacks as inflation increased. In each case, cutbacks caused a recession–a drop in the rate of growth and unemployment–toward the end of the presidential term. This allowed a new president to increase demand and production without new investment by increasing expenditures and extending credit to private enterprise–without dealing with the basic structural defects in the economy. Underlying this recurrent cycle was class conflict between workers and the bourgeoisie to avoid bearing the costs of inflation. Since neither side could completely dominate the process, inflation served as a short-term safety valve or perhaps–to cite Albert Hirschman–an alternative to civil war in Chile (p. 193).

If a great strength of this study is systematic comparative analysis of three Chilean administrations using an innovative neo-Marxist framework, an important weakness of the book is a certain lack of historical perspective with regard to continuity of political issues and debates over Chilean political economy. This is not simply to criticize Stallings for failing to do something she did not intend to do. Clearly this book is not meant as an economic or political history of Chile. Nevertheless, the brief chapter on historical development and structural characteristics (chapter 2) is extremely superficial and contains a number of needless errors regarding the evolution of Chilean political parties, the role of key political figures in Chilean history (for example, Portales, Balmaceda) and the development of the labor movement. These problems detract from the study both for country specialists and for those concerned with the relationship between socioeconomic change and politics more generally.

Paul Drake's *Socialism and Populism in Chile, 1932–1958* (1978), as well as earlier sources, make clear that many of the political issues confronted by the Unidad Popular coalition (e.g., presidential-congressional relations, impeachment of ministers, budgetary control) were frequently faced by nineteenth-century Chilean governments. Obvious parallels exist for the administrations in office from 1938 to 1952. Likewise, the recurrent economic problems Stallings refers to from 1958 to 1973 can be traced to the late nineteenth century and certainly to the post-1930 period.

Perhaps more important than a lack of a longer-term historical perspective is an implicit teleology which is revealed only by way of conclusion: capitalism and reformist development models cannot resolve the dilemmas of capital formation, investment policy, and "demand management" necessary to achieve economic growth and equitable distribution of goods and services in Third World nations. Therefore, according to Stallings, the failure of the three Chilean administrations examined points to the need for a "more solid socialist model" with greater emphasis on investment, project implementation, rationing and collective consumption (p. 127). Though uncertain that a class alliance could be constructed to support this model, Stallings suggests that "political education" and more time to implement rationing mechanisms might pave the way for this more solid socialist model (without elaborating what the empirical character of this model might be).

Notwithstanding Stallings' effort to avoid the debate on the parliamentary versus revolutionary way to socialism (p. 239), or the failure to indicate *how* a more solid socialist model would overcome the problems that frustrated the three Chilean administrations (1958–1973), Stallings astutely summarizes the basic contradictions in Chile between the institutions and practice of multiparty liberal democracy and implementation of *any* integrated development model. She also neatly synthesizes the problems of applying Keynesian solutions, alone, to economic constraints derived from structural defects such as concentration of land and industrial ownership or vulnerability of the foreign sector to international price fluctuations and the caprice of transnational firms. However, Stallings shies away from the logical extension of her findings—Lenin's own conclusions in *State and Revolution* that the construction of socialism requires an end to democracy and overt, harsh repression of the bourgeoisie.

Of course, for those less enthusiastic about such "more solid socialist models," some lessons of the Chilean experience *not* suggested by Stallings are that reform is preferable to brutal military dictatorship; that failure and success in politics and economic policy are relative concepts; that the Christian Democratic reform program, while experiencing some significant failures, did genuinely improve the living conditions of thousands of rural and urban workers, initiated a real agrarian reform, stimulated rural and urban unionization, carried out meaningful educational reforms that expanded the opportunities available to the poorest sectors of Chilean society, and even augmented the Chilean government's share of copper revenues. Civil liberties were generally respected, even if some abuses occurred against striking workers—abuses not unknown in socialist regimes when workers have exercised their "right" to strike. And perhaps a final lesson of the Chilean experience is that conflict over economic development models in the context of liberal democracy produces no "final solutions," no extirpations of evil, and no long list of "economic crimes," even if poverty and misery are not immediately abolished by decree.

BRIAN LOVEMAN

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The State and Society: Peru in Comparative Perspective. By Alfred Stepan. (Princeton, N.J.: Princeton University Press, 1978. Pp. xix + 348. $18.50, cloth; $4.95, paper.)

The main theme of this book is the relationship between political regimes, governments, policies, and organizations, on the one hand, and various social forces (groups, classes, etc.), on the other. Stepan claims that the "relative autonomy" of the political forces in relation to the social forces has been a neglected theme in comparative politics and that his emphasis on this allegedly neglected topic is an important contribution to the field. There is, however, less of a contribution here than meets the eye.

The book is divided into two parts. Part 1 is theoretical and comparative. It contains a survey and critique of three theoretical approaches to "state-society" relations (liberal-pluralist, classic Marxist, and "organic-statist"); a theoretical critique of ideas about corporatism; a typology of corporatist regimes (inclusionary/exclusionary); and a series of hypotheses about the conditions of successful and unsuccessful attempts to install corporatist regimes (not states) in Mexico 1935–40, in post-1964 Brazil, in Argentina 1966–71, and in post-1973 Chile. Part 2 deals with "Peru: An Organic-Statist Experiment" after 1968. In this part Stepan claims that he "elaborates the concepts and arguments discussed in Part One" (p. xv). Actually he largely ignores those concepts as he discusses the Peruvian army as a strategic state *elite*; the relations between the *government* and urban squatters, sugar workers and foreign capital; and the institutionalization (as contrasted with the installation) of organic-statist *regimes.*

Except for certain serious omissions (see below), one of which is critical, the survey of theoretical approaches is useful. The discussion of the concept of organic-statism (pp. 26–45) is particularly valuable; it is too bad this concept is not used to better advantage in the rest of the book. The critique of corporatist ideas is successful to a point, although some of Stepan's targets are sitting ducks and he either distorts them (as with Wiarda) or passes over lightly or ignores most of the ablest representatives of the position he attacks (Morse, Silvert, Paz, Veliz, and several classic and modern Brazilian writers). It is like criticizing dependency writings without addressing the main body of work by Cardoso, Sunkel, O'Donnell, and Jaguaribe. Stepan's disaggregation of corporatism into two types is a useful if limited innovation. Finally, simply as descriptive material, the case study of Peru's inclusionary corporatist experiment is careful and detailed and adds some new information.

The foregoing are, I believe, the main strengths of the book. There are also serious weaknesses in it. Stepan begins by offering an elaborate Weberian definition of the state and by poking fun at contemporary political science for its alleged failure properly to conceptualize and analyze the state (pp. xi–xiv, 3–4). This is plausible and enticing; the reader eagerly anticipates Stepan's promised improvements and innovations. But he does not deliver the goods. After the initial presentation, Stepan rarely refers to the Weberian definition and does not really use it. For him "state" is *not* an important *analytic concept*; it is an *umbrella term* for the political concepts and phenomena that *are* important in his analysis: regimes, governments, policies, organizations, etc. Indeed, by the final chapter, "The Institutionalization of Organic-Statist *Regimes*," the concept of the state—*any* concept of the state—has all but disappeared.

Stepan's real target of criticism, it turns out on closer examination, is not frameworks that say too little about the state, but rather "reductionist approaches to political development" which search for "underlying economic, social, and psychological causes of political behavior." Stepan is unhappy because these approaches assign "little independent weight to the impact of state policies and political structures on the social system" (pp. 3–4). His objections to such writings are valid, but they are also quite familiar, having been made repeatedly and influentially for at least the last 15 years in precisely the comparative politics literature that Stepan describes and criticizes so selectively. And here is where the omission in Stepan's first chapter, which in another context might be merely a surprising but relatively harmless blind spot, becomes a major intellectual gaffe with serious consequences.

Stepan's survey and critique of comparative politics scholarship depicts a field comprised of liberal-pluralists à la Arthur Bentley (!), David Truman, David Easton, and 1960-vintage Almond and Coleman, augmented by classical Marxist theories largely drawn from Europe and Latin America and now by Stepan and "organic-statism." Stepan gives no hint of the intensive and extensive critique of the reductionist literature that took place in the field quite independently of the Marxist, corporatist, and organic-statist approaches. (His oblique, vague, one-sentence footnote reference on p. 4 to "the last few years" hardly obviates this point.) In this critical literature the most important work was by Samuel Huntington, whose 1965 *World Politics* article "Political Development and Political Decay" and subsequent book *Political Order in Changing Societies* (1968) caused an entire field to reassess its premises; but there were many others, including many who earlier had been closer to the liberal-pluralist school. Indeed, by the time Stepan wrote, and indeed much earlier, almost nobody in the comparative politics field was defending liberal-pluralism in the form he depicts it. In this important sense there are strong elements of unscholarly caricature in Stepan's "scholarly" survey.

Stepan's contribution to comparative politics is therefore very substantial if compared to the picture of the field that he presents in chapter 1, but much less substantial if compared to the literature he fails to describe or even to acknowledge. Regarding his findings in chapters 3–7, Stepan writes: "Among the variables found to be particularly useful for predicting the installation of regimes by [corporatist] policies were the *organizational strength* and *ideological unity* of the state elite; the degree of prior development of *autonomous political parties and interest groups;* the degree of *societal polarization*; the extent of *prior social welfare* and other *structural reforms*; and the *ratio of* the state elite's coercive, economic, and symbolic resource *capacity to* effective *demands* made on them by civil soviety" (p. 290, emphasis added; see also pp. 110, 154). This is an accurate brief summary of the main theoretical conclusions of Stepan's book. If you think the concepts he is using and the hypotheses he is proposing have a familiar ring to them, you are right. *Every one of them* was already present in and central to Huntington's 1965 *World Politics* article and his 1968 book. This is not to say that Stepan consciously borrowed Huntington's ideas. It is to say that Stepan's concepts and hypotheses are identical or nearly identical to Huntington's. Moreover, as its title implies, Stepan's eighth and final chapter, "The *Institutionalization* of Organic-Statist Regimes," also uses concepts and advances hypotheses that are very close to if not the same as Huntington's.

The point is not simply that there are random precedents for Stepan's ideas here or there in the history of political thought or in the work of other contemporary writers; important and original contributions often have precedents in that sense. It is that Stepan either does not know or chooses to ignore the state of knowledge in the very literature he takes as his point of departure and to which he seeks to make a contribution. As a result he has restated as if they were new criticisms of the "reductionist" literature that many others (including Stepan himself in a useful article in 1966) have been making for well over a decade, and he has proposed as alternatives concepts and hypotheses that are not new, either. Stepan's book thus contains some of the virtues but also most of the defects (nonoperational concepts, tautological tendencies, aggregation and weighting problems) of Huntington's earlier writings. Stepan is aware of some of these defects (see esp. pp. 108–13, 290–91) but not sufficiently aware. For instance, referring to tests of his five major hypotheses in five country cases, Stepan reports that the results were "in all cases in the hypothesized direction" (p. 290). That means 25 opportunities to reject a hypothesis and not one rejection, and it is not plausible. Nobody ever does that well even in laboratory experiments in the social sciences, to say nothing of the complex real world Stepan writes about.

From the point of view of the general literature on comparative politics, therefore, Stepan has shown once again the utility and the limitations of the kinds of concepts and hypotheses proposed more than a decade ago by Huntington and others. He makes no original contribution either to that literature or to the general theory of the state.

At the level of Latin American political studies, Stepan's discussion of the concept of organic-statism in chapter one is a useful small contribution to the field. If he had consistently employed those ideas in the rest of the book, Stepan might have illuminated their uses and limitations for understanding Latin American politics, and that would have been a major contribution. Instead he tends to slide off them onto the generic Huntingtonian concepts whose uses and limitations are more well known.

ROBERT A. PACKENHAM

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Opposition in Eastern Europe. Edited by Rudolf L. Tökés. (Baltimore: Johns Hopkins University Press, 1979. Pp. xxiv + 306. $22.50.)

It is said that one should not judge a book by its cover. In the example of Rudolf Tökés' *Opposition in Eastern Europe*, this is certainly the case if only because the book's dust jacket prominently features a photograph of Yugoslav dissident Milovan Djilas, none of whose views are discussed in the text that follows. In fact, the book does not even contain a chapter on Yugoslavia. More importantly, the cover is misleading because the very title is not accurate: the "East European" coverage is limited to the four "Northern Tier" states of Poland, Czechoslovakia, Hungary, and the German Democratic Republic, and the word "opposition" is not an entirely appropriate way to characterize the detailed description of dissident activity which is the actual focus of the book.

Like most edited volumes, *Opposition in Eastern Europe* suffers from severe problems of coordination among the chapters. At least part of the difficulty derives from the editor's explicit refusal to impose or even suggest a common definition of "opposition" around which the contributors could structure their essays (p. xxiii). Thus the authors describe a broad range of activities from open defiance of the regimes and outright rejection of their ideological foundations to the role and status of human rights to "para-opposition," a tolerated form of questioning regime principles and policies in Hungary. It is true that these activities are all oppositional in the common dictionary sense of the word, but in a work of political science one could expect a bit more rigor. In the editor's defense, it might be noted that he also explicitly rejects any scientific approach to his subject: "Instead of elaborate taxonomies and social science models of unorthodox political behaviour in Eastern Europe, all contributors sought to provide carefully researched country and case studies to inform the non-specialist reader rather than those with preference for abstract theorising from insufficient and still unfolding data" (p. xxiv). This statement smacks of *post hoc* justification for including between two covers a set of quite disparate essays on what is certainly "unorthodox political behavior" in East European terms rather than a carefully thought-out standard for editorial guidance.

While the work as a whole is a disappointment, several of the essays are a delight to read. It redounds to the editor's credit that he assembled such a uniformly excellent group of experts on the region. The four country chapters are thoroughly workmanlike chronicles of dissident and oppositional activity in the Northern Tier, and the essay by Alex Pravda on patterns of dissent, opposition, and accommodation among industrial workers is a first-class contribution. Tökés' own chapter on human rights in Eastern Europe is a thoughtful and provocative piece, as is Ivan Szelenyi's somewhat dense overview of the ideological and philosophical currents among intellectuals in the region. Paul Lewis' chapter on opposition among the peasantry (which essentially concludes that there is none) is a useful study of a social group which receives too little attention.

JAMES P. MCGREGOR

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Soviet Intervention in Czechoslovakia, 1968: Anatomy of a Decision. By Jiri Valenta. (Baltimore and London: Johns Hopkins University Press, 1979. Pp. xii + 208. $12.95.)

The decision of what to do about the 1968 Czechoslovak experiment with "socialism with a human face" (when Dubček and his associates persisted in their reformist course despite the ever more insistent Soviet warnings) was undoubtedly the most agonizing dilemma the Soviet Union had to cope with in Eastern Europe since Tito's defection in 1948. What prompted the Soviet Politburo, after having seemingly worked out a compromise in its unprecedented meeting with the Czechoslovak Communist party leadership at Čierna, to opt for a military resolution of the crisis, even though it knew full well how adversely it would affect the Soviet Communist party's relations with the "fraternal" communist parties of many other countries?

This monograph is an inquisitive scholar's search for as definitive an answer to this intriguing question as can be arrived at without having access to confidential Soviet documents. Employing the bureaucratic-politics paradigm as a methodological tool, Jiri Valenta explores and evaluates the whole range of factors he considers as having been instrumental in shaping the fateful Soviet decision to crush the "Czechoslovak Spring" by armed intervention. He assesses the respective roles played by various members of the Soviet Politburo, Central Committee and Secretariat, the Soviet military, the communist leaders of East Germany, Poland and Hungary, the Soviet Ambassador to Czechoslovakia, and the anti-reformist coalition within the Czechoslovak Communist party. He takes into account the Soviet fear that the Czechoslovak reformism would provide impetus to polycentrist and autonomist tendencies in Eastern Europe and in the Ukraine and that it might bring about closer ties of Czechoslovakia with Yugoslavia and Rumania in a sort of a new "Communist Little Entente." He considers at some length the part played by the overcautious attitude of the United States which, "far from deterring the Soviet Union from military action, had the opposite effect of giving a powerful argument to the interventionist coalition" (p. 132). He sees an important pro-intervention factor in the Soviet perception that the Soviet invasion would not be met with armed resistance.

The monograph is well conceived, ably written and richly documented. Although a good part of the evidence is necessarily inferential and possibly also colored by personal predilections of some of the many interviewees that Valenta had consulted, it is a valuable contribution to our understanding of what happened, and why, in those crucial months of 1968. While I agree with most of the author's conclusions, there are two items which appear rather questionable: (1) Was there really at any time of the crisis any prospect that the Czechoslovak army would be ordered to resist the Soviet invasion, as the author implies for the period prior to June 1968? (p. 129). (2) Did the fact that the Soviet leaders were receiving biased information about political realities in Czechoslovakia play any truly significant role in their decision-making process, as the author seems to suggest? (pp. 124, 156).

EDWARD TABORSKY

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The Catholic Church, Dissent and Nationality in Soviet Lithuania. By V. Stanley Vardys. (New York: Columbia University Press, 1978. Pp. xiii + 336. $22.50.)

A close relationship between religion and nationality in a number of countries could easily be proven in the contemporary world. One should not specifically refer to Islam or Khomeini to verify the proposition that nationalism, in some cases, is generated by religious factors, or that religion within an ethnic-national context has become imbued with elements of nationalism.

The examples of this are particularly prolific in the borderlands, multinational states and areas of religious pluralism. V. Stanley Vardys has produced an impressive study of this phenomenon based on the Lithuanian case. He focuses primarily on the intertwining of nationality and religion in Soviet Lithuania, i.e., since 1944, yet he also discusses the politico-religious history of Lithuania and shows the role of Roman Catholicism in shaping the Lithuanian nationality in the nineteenth and twentieth centuries.

After having taken over Lithuania in 1940, and reconquering it in 1944, the Soviet Russian regime faced two formidable "enemies" in that non-Slavic, strongly nationalistic and pro-Western country. These "enemies" were the secular nationalist intelligentsia with a supportive base in the rising middle class and the quite progressive peasantry, and the Catholic Church, based on its solid constituency of the committed clergy, intellectuals and the church-obedient masses in a not-yet secularized agrarian society.

Although the first group, still more politicized, paid a high price during the occupation (deportations, reprisals, defections to the Moscow side), it yet demonstrated sporadic armed resistance in the form of anti-Soviet guerrilla warfare from 1944 until 1952. The Catholic Church, the religion of 85 percent of Lithuanians, on the other hand, although not spared by the regime during the years of pacification, later became the only catalyst of Lithuanian religious and national aspirations. In the 1970s, the Church began to fill this role for secular segments of Lithuanian society as well.

The status and function of Lithuanian Catholics, unique among the religious bodies in the USSR, and comparable only to the Polish Church, is scrupulously researched and abundantly documented in Vardys' volume. *The Chronicle of the Catholic Church of Lithuania,* a *Samizdat* periodical (30 issues between 1972 and 1977), plus other underground publications and reports were essential sources for the author, yet the official Soviet sources were also thoroughly investigated. As a result, the book is a mine of information and data which offer the opportunity for serious comparative analysis and interpretation.

I would have desired more information on the possible coalition the Lithuanian Catholics forged, both at home and in Soviet exile, with secular circles, as well as on the present status of the remnants of Polish clergy in Lithuania. Where do they stand now in the nationalist-oriented resistance of the Lithuanians? Also, an attempt to survey the Lithuanian situation against a broader background of religious resistance in the Soviet Union (Lithuanian Lutherans, Latvian and Ukrainian Catholics, Russian Orthodox believers, Jews, etc.) could have opened an additional dimension to the study. Also, the impact on Lithuania of the Polish Church and its experience under communism could have been elaborated more extensively.

Vardys does not apply sophisticated methodology in analyzing and evaluating his data and findings. It seems that he did not assign himself to such a task. A short concluding chapter reiterates the author's well-founded propositions about the misconceived Soviet notion of the Church-state separation, and the convergence of religion and nationalism at a stage when a small and dynamic ethnic community clings to its survival.

A valuable selection of legal sources pertaining to the Soviet religious legislation, plus a comprehensive bibliography and a number of documents of the Lithuanian religious *Samizdat* (petitions, memoranda, reports) perfectly supplement the book.

In all, Vardys' book is a well-researched and useful addition to the growing body of scholarly literature on religion under communist rule.

VASYL MARKUS

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The House of Commons in the Twentieth Century: Essays by Members of the Study of Parliament Group. Edited by S. A. Walkland. (New York: Oxford University Press, 1979. Pp. vi + 649. $55.00.)

This lengthy and expensive volume traces in often excruciatingly minute detail twentieth-century procedural developments in the British House of Commons. Produced by the members of the Study of Parliament Group—the informal group of British academics and House of Commons clerks who meet to discuss and analyze the Commons—these essays take the reader sometimes year by year through changes in the parliamentary parties, the whips, the office of the speaker, the committees, etc. Clearly only a heavy-handed editor kept a chapter on parking facilities for MPs in the twentieth century out of the book.

Because of their great detail, these ten chapters and two appendices are valuable as a reference tool. Every scholar with a passionate interest in the House of Commons will want this book. Most of the information has been written elsewhere, but the book is a handy compendium. Its value to the general reader or to someone who wants to grapple with larger issues of parliamentary developments and British politics is questionable, though.

The starting point for the authors is the gap in descriptive material on the Commons in this century. For the nineteenth century, the great Austrian scholar Josef Redlich's three-volume work on the Commons is an extremely useful and insightful work. The authors note that no follow-on volume to Redlich has been produced, and offer their essays as an update. There is one major difference between Redlich's task, though, and that of the authors of the present study: he had major transformations in the nineteenth-century parliament to describe and analyze, especially those following the Great Reform Act. The House of Commons at the beginning of the twentieth century when

Redlich wrote was a very different beast from the Commons at the beginning of the nineteenth.

Viewers of the twentieth-century House of Commons have no similar set of changes on which to focus. The Commons of this century has changed little, though change swirls around it. Two world wars, the dissolution of the British Empire, the growth of the welfare state, the rise of a new political party and the virtual end of another, entry into the European Communities—all these have come about with the basic procedures and functions of the House of Commons basically the same as they were when Redlich produced his work. As Stuart Walkland points out in his introduction to the present volume, the House of Commons "essentially remains an assembly which Balfour would immediately recognize" (p. 5).

Scholars concerned with British politics would have been better served by the authors had they turned their attention to why the House of Commons has remained so static. Walkland and his contributors make passing remarks on the subject, but there is no attempt to address the issue systematically. And that seems to be a much more important and useful task than simply reemphasizing in chapter upon chapter that this institution has not moved. For instance, minor changes in procedure for consideration of private members' legislation are discussed at length in Peter Richards' contribution, but without any analysis of the factors contributing to making private members' bills still so unimportant in the Commons when it appears that in some other parliamentary systems, individual members' initiatives are becoming more important.

Nevil Johnson's chapter on select committees stands out for his attempt to look beyond the simple procedural changes in select committees, especially those of the late 1960s and early 1970s. (Johnson does not address, of course, changes in the committees that have developed since the advent of the Thatcher government. Some observers are speculating that these changes might be far more substantial than what has occurred the previous 15 years, though I believe Johnson would be dubious.) Johnson argues, at most times convincingly, that changes in procedure that ostensibly meant a great deal in terms of an increased role for the select committees in practice meant little because the underlying principles of the House of Commons had not changed. The Commons with an expanded select committee system, as without it, is an institution dominated and controlled by the government, often in cooperation with the Opposition front bench. This control group has no real interest in and no desire to accede to the rambunctious backbenchers who sit on select committees and spend most of their time taking aim at governments present and past.

Johnson goes so far as to argue that those parliamentary reformers who believe that select committees are a meaningful way to give the Commons a substantive role in British policy making may have it wrong. While one might disagree with his conclusion and the reasoning leading to it, the fact remains that Johnson goes beyond mere factual presentation of procedure and what it is supposed to mean to inquire as to what it really does mean and how important it really is. These larger questions are significant, especially because for those in the British political system, and for far too many of those who study it, procedure is everything. Writers on British politics take as an article of faith Sir Kenneth Pickthorn's maxim that procedure is the only constitution that Britain has. So analyzing British political institutions becomes using procedure to describe them and using procedure to explain them. Johnson goes beyond this in asking what difference different procedures make if they are taken in the context of underlying constants in the political system. His answer, not surprisingly, is that they make little difference.

The other authors do not generally look at these larger questions, and so one is left with a basic recitation of facts that are not particularly enlightening. The absence of any serious attempt to integrate the changes—or lack thereof—in the various parts of the whole devalues the work even further. Changes in the recruitment and socialization of MPs, e.g., more full-time MPs, are related to the growth of select committees, or so one would hypothesize. Very few attempted connections of this sort appear in these writings, however, leaving one with the even stronger feeling that procedure is being studied for its own sake.

The authors of this volume did not set out to write a social-science analysis of the House of Commons. Their more limited task and result are only reflective of a larger trend in British social science to be descriptive and not analytic. Perhaps the best response is to sit back and be thankful that some people continue to be so fascinated with procedure in the House of Commons that they can spend 80 pages writing about a largely ceremonial office, that of the Speaker.

HARRIS N. MILLER

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G. D. H. Cole and Socialist Democracy. By A. W. Wright. (Oxford: Clarendon Press, 1979. Pp. 301. $36.00.)

Is it possible for a lecturer in political studies to write an objective intellectual biography of a man who once said, "The deepest insult you can offer me is to call me a 'social scientist' "? A. W. Wright's references to Cole's "sloppy intellectual pluralism" and "shabby apologetics" might at first lead one to wonder. Yet this is a wonderful and delightful volume, drawing the reader inexorably deeper into the fascinating contradictions that made Cole one of the leading socialist theorists of this century.

These contradictory tensions—between liberalism and socialism, constitutionalism and revolution, individualism and collectivism, nationalism and internationalism—retain a surprising vitality in the modern world. Indeed, the rediscovery of G. D. H. Cole rests in no small part on the contemporary revival of the workers' control movement in England and Europe, breathing new life into the body of thought represented by guild socialism and the concepts for which Cole stood.

If anything, Wright makes too much of the opposing forces which shaped Cole's beliefs, seducing the reader into the supposition that Cole played more in these fields than others. Compared to Harold Laski, however, the contradictions in Cole may be seen as warp and woof of the times. It is not so much that Cole played more, but that he played better.

Referring to Laski's attack on the jurisprudential fiction of state sovereignty and to the doctrine of social pluralism, Wright notes of Cole and guild socialism, "It was intellectual enough to be sensitive to the philosophical argument, but worldly enough to emphasize the centrality of industrial action. The intellectual appeal of guild socialism was strengthened by its consonance with a prevailing academic temper, yet it was not itself academic" (p. 34). Where Laski, who ironically had far more influence on American political science, was unable to sustain a positive vision of social coordination following his devastating attack on jurisprudential political science, Cole was successful. (See Garson, *Group Theories of Politics*, Sage, 1978, pp. 17–23, 48–51).

Cole's guild socialism poses basic and profound questions for American political science. It does this through a romantic model of the state, one which contains a beguiling normative model of social coordination that has never been adequately addressed by American political scientists. Only recently had scholars of the pluralist tradition come to consider the challenge worthy and timely.

The questions Cole raises center on the assumptions about democracy most widely held in the West. It is assumed, for example, that classical, rational models of democracy may be remolded in a pluralist group context. Cole questions whether a group basis of politics can be considered an adequate model when the theory makes no provision for the democratization of its constituent building blocks. It is also widely assumed that the distinction between public and private is a substantial one and that democratic and organizational theories must provide different rules of operation for the two. Cole, however, seeks to provide a model of social interest which transcends this distinction.

Wright is scarcely concerned with the implications of Cole's thought for political science. Instead his fascination lies with the implications of romanticism in politics, not with the logic of social models. In Cole he has found an ideal subject. Cole's first book was a work of poetry, and between 1910 and 1915 he edited the *Oxford Poetry* anthologies. It is Cole's genius to have predicated his theories of socialism on humanistic and aesthetic principles rather than economic ones. Consequently, his work is addressed not so much to the construction of a new social order as to founding the environment for new social relationships. This is a point of view considerably in advance of its times, at once borrowing from utopian and collectivist strands of socialist theory but ultimately making up a different sort of social fabric. It was neither the philosophical socialism of a Tawney, with his bland acceptance of bourgeois democratic machinery, nor the corporatist brand of socialism into which Laski lapsed in his later years.

"The familiar brands of collectivist socialism were somehow things one wanted for other people rather than for oneself," Cole wrote (p. 268). The fellowship of guild socialism, however, "offered me a kind of socialism that I could want as well as think right." In psychological terms, Cole was rejecting the forms of socialism which addressed purely economic needs and instead aspired to a system providing for the higher levels of Maslow's need hierarchy—for social esteem and for self-actualization. In this way Cole should be required reading for every student of political psychology.

Wright correctly criticizes Cole for his neglect of the economics of labor-managed economies and even the design of social planning in his socialist state. Cole was not a Renaissance man, uniting art and engineering. He was, as Wright states, a romantic—but a romantic with a difference. Cole ultimately believed in the democratization of aesthetically actualizing relationships. That is the underlying contradiction of G. D. H. Cole, and the one so eloquently treated by the author.

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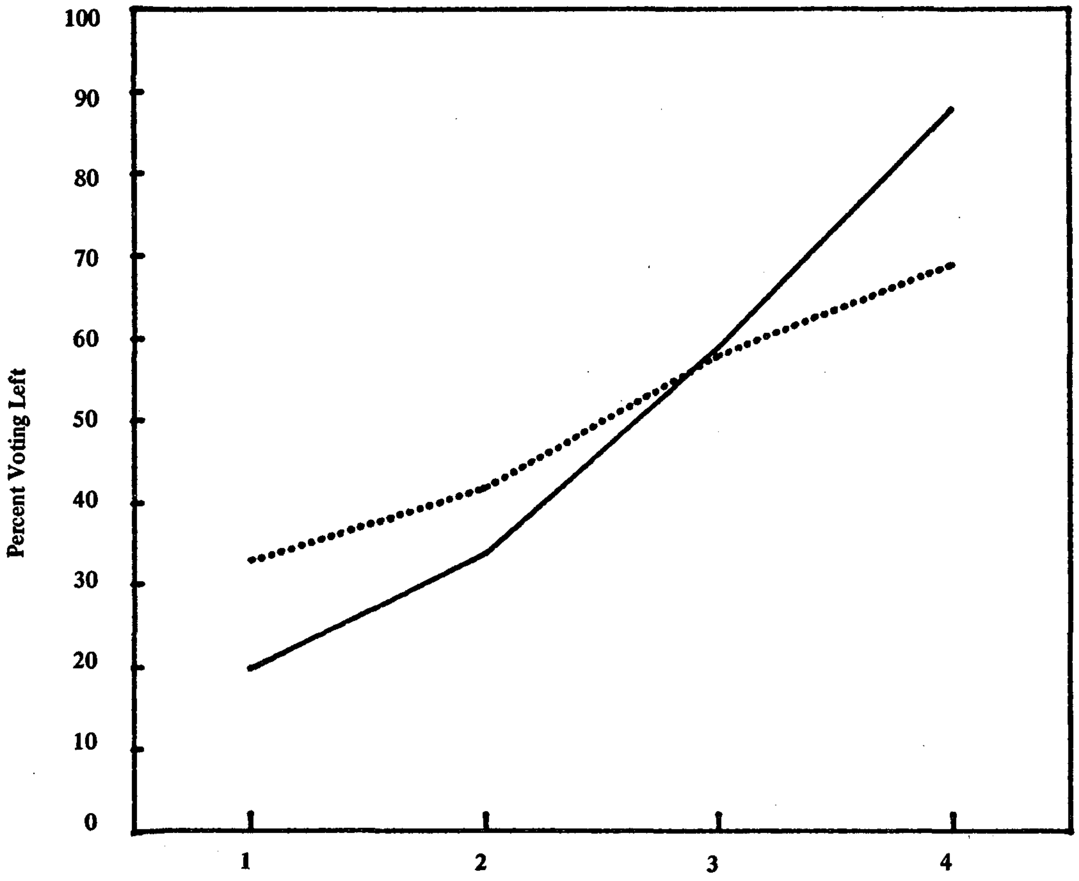
Conflict and Consensus in France. Edited by Vincent Wright. (Totowa, N.J.: Frank Cass, 1979. Pp. 150. $19.50.)

This is a collection of nine articles, each by a different author, that first appeared as a special issue of the journal *West European Politics.* The contributions include: "The Two Frances: The Historical Debate" by Douglas Johnson; "Legitimacy, Democracy and Consensus: A Presidential Analysis" by John Frears; "The French General Election of March 1978: 'La Divine Surprise' " by editor Vincent Wright; "Dissentient France: The Counter Political Culture" by Jack Hayward; "Politicisation and Cohesion in the French Administration" by Anne Stevens; "Individualism Versus Collectivism: Economic Choices in France" by Diana Green; "Higher Education in France: A Two-Track System" by Ezra Suleiman; "The Political Imperatives and Dilemmas of French Defence Policies" by

Factors in the Partisan Vote (France, 1978)



———— Religion

1. Regularly practicing Catholic
2. Occasionally practicing Catholic
3. Non-practicing Catholic
4. No Religion

.... Occupation of Head of Household

1. Upper Management, Industrialists and Liberal Professions
2. Shopkeepers and Self-Employed
3. Middle Management and White Collar
4. Workers

Source: Guy Michelat and Michael Simon, *Classe, Religion et Comportement Politique* (Paris: Presses de la Fondation Nationale des Sciences Politiques and Editions Sociales, 1977), p. 39.

Dorothy Pickles; and "All Jacobins Now? The Growing Hostility to Local Government Reform" by Howard Machin.

It would not be possible, in any case, to give fair treatment to all these articles within the compass of a brief review, but my attitude toward this book is so colored by my reaction to one contribution that I will confine my comments to that chapter alone. The chapter in question is Wright's discussion of the 1978 election, the longest in the book and, because of the centrality of its subject, likely to be the most widely read as well. Wright makes more than one misleading statement in this chapter, but one of his comments involves such a glaring misreading of his own evidence on a point of basic importance to an understanding of French electoral behavior and its relationship to elite political conflict that it must be discussed in detail.

On p. 39, Wright reproduces a table that appeared in *Le Nouvel Observateur* shortly after the 1978 election reporting the distribution of the partisan vote by various demographic factors, based on a SOFRES post-electoral national sample survey. On the same page, Wright writes, "Class dominates, but religion remains a powerful factor: the vote of the Left varies in inverse proportion to the degree to religious practice." A footnote to that sentence refers to a book published by two French scholars in 1977. Now, neither the book Wright cites nor the table he reproduces supports his statement that class predominates over religion as a factor in French electoral behavior. The book (Guy Michelat and Michel Simon, *Classe, Religion & Comportement Politique,* Paris, 1977) examines class and religion, separately and jointly, from every conceivable viewpoint allowed by the available data, and concludes that religion predominates over class in determining French electoral behavior.

The table permits us to compare the effects of religion with those of class (defined on the basis of the occupation of the head of household) on a grouped basis. For religion, the table contains four categories: regularly practicing Catholics, occasionally practicing Catholics, non-practicing Catholics, and people without any religion. With regard to the occupation of the head of household, if we ignore farmers and inactive or retired people, whose class status is indeterminate, we are also left with four categories: upper management, industrialists and the liberal professions; shopkeepers and the self-employed; middle management and white collar workers; and workers. We therefore have four categories of religious attachment and four categories of objective class.

Concerning the partisan vote, the table reproduced by Wright contains seven partisan categories that can easily be grouped into two categories of left and right. We will define the left generously by including candidates from the ecology movement, a women's liberation movement, and opposition Gaullists. The figures can be tallied in seconds from the table in the book. They show that the proportion of people voting for the left varies, across our four-point measure of class, from 33, to 42, to 58, to 69 percent, as we move from high to low on the occupational scale. On the four-point religion scale, the proportion of people voting for the left varies from 20, to 34, to 59, to 88 percent. On this grouped basis, it is clear that religion was a more powerful factor in the vote in 1978 than class, at least when class is defined in occupational terms. There is a monotonic progression on both scales, but the slope is steeper for the religion scale than it is for the class scale, a point that is demonstrably clear from the accompanying figure.

I am not suggesting we foreclose discussion about the relative importance of class and religion as factors in French voting. There have been no detailed individual-level studies of those factors based on data collected after the late 1960s, and it may well be that class has gained in importance relative to religion since the death of de Gaulle. But the data that we do have, in the form in which they are available, indicate that for 1973 and 1979, as well as for 1978, religion is a more important electoral factor than class.

It is unfortunate that Wright's error, along with numerous misprints scattered throughout the volume, detract from the merits otherwise displayed by his own chapter, as well as by the other contributions to this collective work.

ROY PIERCE

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International Politics

Tariffs, Quotas and Trade: The Politics of Protectionism. By Walter Adams et al. (San Francisco: Institute for Contemporary Studies, 1979. Pp. xiii + 330. $6.95, paper.)

The title suggests a study of the political process of commercial policy formation, but in this volume only a few of the 16 articles by 19 authors are related to this general subject area. Much of the book consists of a pot-pourri of nontechnical summaries of diverse topics in international economic policy: the "new international economic order," arguments for restricting outward direct investment, the international monetary system and its reform, fixed versus flexible exchange rate systems, international commodity agreements, the North-South dialogue, the prognosis for the U.S. agricultural surplus, OPEC and the U.S. dollar exchange rate, etc.

The preface asks whether there exist new arguments for protection (i.e., other than the optimum tariff to exploit monopoly power, national defense, nationalistic arguments), but none of the authors directly answers the question. If a new argument for protectionism were to emerge from such a study, it might be based on a distinction between maintaining a system of free trade and dismantling trade barriers already in place. Although the conventional long-run analysis concludes that free trade is a first-best solution in terms of a nation's welfare, it may not necessarily be desirable to move from a tariff-laden economy to free trade; it is an empirical question whether the discounted stream of benefits from tariff removal outweighs the associated adjustment costs. However, none of the authors satisfactorily presents new evidence or summarizes recent research on the welfare effects of tariff removal.

Cuddington and McKinnon develop the thesis that in times of high unemployment, conflicts among special interest groups (i.e., questions of income distribution) become less important, so that changes in tariff policy leading to increases in total employment will meet with less resistance than they would in a period of low unemployment. It seems that the relationship, if it exists, is more likely the opposite. A multilaterally negotiated round of tariff cuts might boost export-related employment more than enough to offset the loss in employment related to import-competing production. However, if an already depressed import-competing industry were to face further cutbacks in demand because of reduced tariff barriers on competing imports, adjustment costs for the import-competing industry would likely be greater than in a period of full employment and bouyant output demand, and resistance to tariff-cutting might reflect the unfavorable timing of tariff reductions.

Amacher et al. provide a brief overview of some political determinants of trade policies, distinguishing among the interests of producers, consumers, and politicians; and it reflects the inadequacy of our knowledge in this area. Their suggestion that a successful industry lobbying effort is associated with concentrated industries seems questionable, as they mention one example (steel) and one counter-example (textiles). Obstacles to consumer lobbying efforts are discussed, but the link between congressional lobbying effort and trade policy influence is not explored. Also, while the discussion of the determinants of trade policy is welcome, it is not closely related to the subsequent discussion of the changes in U.S. trade policy toward greater protectionism since the Kennedy Round.

Deardorff and Stern discuss several influences of international trade on workers, asserting that a worker's attitude toward protectionist trade policy reflects the net effect of the various influences (on consumer prices, and on the demand for labor in his or her occupation and in the industry and region where he or she works).

Although Sweeney in the last chapter attempts to weave a common thread through the preceding chapters, the synthesis cannot be achieved. The curious result is an exhortation to solve problems of U.S. foreign economic policy by putting a halt to the improvident fiscal and monetary policies pursued in recent years. It is curious because, the villain having been thus identified, the questions of trade policy which ought to have been central to the book presumably fade into insignificance.

WAYNE LEWIS

International Monetary Fund

Understanding Foreign Policy Decisions: The Chinese Case. By Davis B. Bobrow, Steve Chan, and John A. Kringen. (New York: Free Press, 1979. Pp. xii + 242. $16.95.)

The eternal truth in international affairs is that other peoples and their cultures are foreign. Consequently, many of the problems encountered in foreign policy are explainable not by a lack of, or poor, communication, but by an inadequate understanding of others and their ways. Feeling that no one theory of Western decision making could adequately explain decision making in other cultures, Davis Bobrow, Steve Chan, and John Kringen researched the foreign policy process of the People's Republic of China. In designing their research, they chose the mediated stimulus-response model of decision making as the theoretical framework and assumed that members of the elite are the essential actors sharing a more or less common memory, interpretation, and policy preference.

Given the assumption of cultural difference, it was important for the authors to show that the Chinese, in particular those of the People's Republic, are different. Using data collected in both China and the West on major international conflicts between 1959 and 1968, they demonstrated differences in the level of attention paid to them and in the perception of these events. A further difference was demonstrated through a semantic analysis—the Associative Group Analysis—between Chinese raised on the mainland and those raised in Hong Kong. It was shown that those raised on the mainland had been politicized differently. The conclusion drawn from these differences was that the People's Republic of China would develop and use unique decisional processes.

After developing a set of decision-making rules for the Chinese, the authors used the data from the Psyop Management Information System (PAMIS)/Foreign Media Analysis File to test propositions on Chinese treatments of events and nations. These data were generated by a content analysis of the *People's Daily* from January, 1972 to October, 1974. Using 30 events and four measures, the authors tested eight hypotheses on treatment of events that predicted subsequent news coverage in the *People's Daily*. Their results were mixed—good predictions for some, but less so for others.

The treatment of nations by the *People's Daily* was presented first for all the countries and then for those who were identified as adversaries. The first test was whether classification schemes for nations developed by Lin Piao and others were being used by the Chinese to describe other countries. Since coverage in the *People's Daily* was not even, only the data on the 40 leading countries were O-factor analyzed. The results gave five clusters of nations that correspond to the five projected classifications from "socialist" to "enemies." Looking in more depth at the four countries in the "enemies" category, the authors found that their treatment more or less followed expectations except for the Soviet Union which presented obvious difficulties for the Chinese. The book concludes by summarizing insights on Chinese decision making into a model with predictive potential.

This is a valuable book establishing much of what the authors set out to show. In today's shrinking world, any work on cross-cultural decision making would be important, but the theoretical approach to Chinese decision making places this book in a unique category. The results also show once again that the mediated stimulus-response model of decision making is a theoretical framework that produces significant insights into the foreign policy-making process.

Yet however valuable, the book does have some blemishes. First, the writing, especially in the beginning, seems to have been done by a committee dedicated to finding the most difficult, jargon-ridden expressions. Second, in terms of theory, it would be difficult to judge the results of an effort "to derive falsifiable predictions" (p. 3) and to test them, when the propositions are set within an explanatory context that accepts bimodalities—"rules and maxims whose key terms are opposites to be applied concurrently" (p. 56). Fortunately, the empirical tests were not dependent on using bimodalities. Third, in stating their predictions, the authors were not as consistent as they should have been in referring to what is predicted. In setting their research, they originally talked of dealing with "beliefs pertinent" to the end results of policy making or policy implementation (p. 4). Yet later, they stated "we want to link beliefs to policy practice" (p. 137). So it becomes unclear whether they are predicting beliefs, policy, or behavior. Empirically, they are predicting newspaper coverage. There are no data presented on actual Chinese behavior and probably none was intended to be. By not being consistent in referring to what is predicted, they arouse expectations that cannot be fulfilled. While these blemishes reduce the sweep of this work, they cannot diminish the value of what is presented.

THOMAS J. PRICE

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Calling a Truce to Terror: The American Response to International Terrorism. By Ernest Evans. (Westport, Conn.: Greenwood Press, 1979. Pp. x + 180. $19.95.)

In this study Evans seeks to describe terrorism as an international political question, the U.S. response to the problem, and how flaws in that policy could be overcome. He looks at the causes and strategies of several major international terrorist groups, and discusses some of the international difficulties they can create.

Ultimately, Evans' book must be judged on its own terms—that of critiquing U.S. policy, which is the subject of the second half of the book. Unfortunately, it is here that the work fails. The book's few minor factual flaws can be forgiven—what is more difficult to justify is a fundamental misunderstanding of the foundations, complexities, strategies and tactics of the U.S. response to terrorism. The work is all the more disappointing when one notes the wealth of interviews—including several with U.S. policy makers—Evans conducted (although his failure to interview any Arabs leaves him open to charges of obtaining a one-sided view of the situation in the Middle East).

In what can only be termed amazing for one who has worked in Washington after leaving Cambridge, Evans fails to note that American foreign policy on terrorism comes under the purview of several federal, state, and even local agencies, all with differing goals which frequently clash. He chooses to attack what is really only a part of the State Department's policy on terrorism, and is silent on, *inter alia*, the work of the Departments of Defense and Commerce, the Federal Aviation Administration, the Nuclear Regulatory Commission, the Central Intelligence Agency, and even his former employer, the Congress. One searches in vain for an account of the problems in establishing a quick-response anti-terrorist paramilitary squad (on the model of the West German and Israeli experiences), the development of anti-hijacking measures, protective security, the difficulty in obtaining and analyzing intelligence on the movements and plans of terrorists, the problems in safeguarding nuclear materials from terrorists, or congressional attempts to halt patron-state support of terrorists.

These sins of omission are compounded by sins of commission in discussing U.S. State Department policy. Although Evans promises to look at U.S. policy throughout the decade, he limits his discussions of incidents, negotiation policy, and multilateral conventions to the first half of the decade. In light of the major review of U.S. policy by the Carter administration as reflected in Presidential Review Memorandum 30, this narrow focus limits the book's utility.

The discussion of deterring hostage-taking takes us no further than what the Rand Corporation's statistical analyses demonstrated five years ago. Evans notes that our "no concessions" policy aims at refusing to grant terrorists' overt demands for prisoner release or monetary ransom, and thus deny them their reason for attacking. He rightly notes that U.S. citizens have nonetheless continued to be taken hostage by terrorists, and argues that one reason for this may be that terrorists seek publicity for their cause, and can more easily obtain it by attacking Americans. What he fails to note is that although official U.S. policy is to refuse to grant concessions, American corporations—which have become the more frequent target of kidnapers overseas—are under no such restriction, buy kidnap insurance, and very quickly give in to ransom demands. Moreover, Evans argues that as our policy has not been an effective deterrent, the policy should somehow be changed to allow for the possibility of tactical concessions on a case-by-case basis. While this may permit greater bargaining flexibility, the suggestions raise two questions. First, it is impossible to determine how many kidnappers were in fact deterred by the "no concessions" policy. Assuming ceteris paribus, is it not possible that granting concessions would have led to even more incidents of hostage-taking? Second, would an announced policy of granting concessions selectively lead to more kidnappings? Would not the terrorists be tempted to believe that perhaps their demands would be granted? Moreover, Evans is exceedingly vague about what would constitute a "political response" to terrorist incidents. Would it only involve selective concessions?

Evans makes a crucial conceptual error by arguing that the U.S. distinguishes between one-step acts of terrorism (i.e., simple bombings or arson attacks) and two-steps acts (i.e., hostage-taking), and that multilateral conventions have aimed at deterring the former. A simple reading of the legal history of these conventions will establish that these treaties deal with hijackings, attacks on diplomats, and hostage-taking—all examples of two-step acts. American attempts to prevent the single-phase attacks focus on improving physical security and acquiring advance warning through intelligence.

Evans' analysis of the U.S. attempts to combat terrorism in multilateral forums is limited to the debate on the 1972 U.S. draft convention on terrorism. He ignores the International Civil Aviation Organization's handling of crimes against civil aviation, and how its three conventions served as models for anti-terrorism legislation by the United Nations, the Organization of American States, and the European Community. Evans chides the U.S. for referring to several terrorist acts as "common crimes," and interprets this as indicating political naivete—i.e., that the U.S. fails to recognize the political nature of terrorism. This attribution confuses beliefs with tactics. What the U.S. has attempted to do is to limit the "political offense exception" to extradition treaties—a clause which allows a nation to refuse to grant extradition for offenses which have political motivations. The U.S. has attempted to incrementally chip away at the class of crimes which can juridically be considered political. Hence, hijacking, hostage-taking, and attacks on diplomats, whatever their motivations, are now under international law not to be considered exempt from extradition treaties. Countries must either extradite or prosecute individuals accused of these crimes. Legally reintroducing political considerations would prove counterproductive, allowing terrorists to go free—certainly not an effective deterrent.

During the past decade, a welter of self-appointed experts have arisen to cash in cynically on the fight against terrorism. In contrast, Evans is genuinely concerned with making society safe from terrorist depredations. It is thus doubly unfortunate that his book falls short of his commendable aspirations.

EDWARD MICKOLUS

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Strategy and Security in Northeast Asia. Edited by Richard B. Foster, James E. Dornan, Jr., and William M. Carpenter. (New York: Crane, Russak, 1979. Pp. xv + 315. $19.50.)

This volume contains 15 papers presented at three international symposia held in Seoul, Tokyo, and Washington in 1977 under the auspices of the Strategic Studies Center of SRI International (SSC/SRI), the Asian Institute for Public Policy (Seoul), and the Japanese Committee on Defense Policy Analysis (Tokyo). Added to these papers are five articles which were published in the first issue of the SSC/SRI journal, *Comparative Strategy*, in September 1978. As in most edited conference collections, the selections in the volume vary in quality of scholarship.

Among the several themes discussed by the contributors, no single issue is as extensively discussed as the Carter administration's announced plan to withdraw U.S. ground troops from South Korea for the obvious reason that the implementation of the plan would inevitably affect the existing balance on the Korean peninsula as well as the region as a whole. Thus if there is any unifying theme in the book, it is that of the U.S. troop withdrawal and its implications for the existing strategic equilibrium of the region.

There is a general consensus among the contributors that the Carter administration's announced plan to withdraw the U.S. ground troops by 1982 is ill-timed and unwise, for it would not only enhance the danger of war on the Korean peninsula, but also adversely affect the security interests of the U.S. and its allies in the region. Furthermore, Foster and Carpenter argue that (1) it would result in global perceptions of a further U.S. retreat; (2) it would enhance Chinese fears of Soviet encirclement and possibly drive Peking to seek rapprochement with Moscow; (3) Japan may be either driven to a further accommodation with Moscow or may opt to go nuclear; and (4) similar reactions are possible in Korea and Taiwan (p. 48). Harold C. Hinton's analysis of the Chinese attitudes toward the troop withdrawal issue plus perspectives of several Japanese contributors (e.g., Hitoshi Hanai, Hideaki Kase, I. Sugita) tend to substantiate the second and third points mentioned by Carpenter and Foster. To be sure, none of the contributors advocates the permanent stationing of the U.S. ground troops in Korea. However, what is stressed is that it should be postponed until certain basic preconditions are fulfilled, such as the modernization of the South Korean forces, rapprochement between Seoul and Pyongyang, guarantee of the independence and security of the two Koreas by the four major powers, and simultaneous admission of them to the U.N. (pp. 136, 164).

Contributions by Kyung-Won Kim, Chong-Whi Kim, and Sang-Wu Rhee are useful in understanding Korean perspectives on the troop withdrawal issue, while Kase, Hanai, and Sugita provide Japanese viewpoints on the same topic. James Dornan, Richard C. Stilwell, and Osamu Miyoshi discuss not only the changing security environment of Northeast Asia, but also necessary measures to cope with the threat posed by the growing Soviet military power in rhe region. Both Miyoshi and Stilwell emphasize the importance of (1) maintaining the credibility of the U.S. security guarantees to its allies; (2) increasing the military capabilities of U.S. allies, particularly Japan; and (3) defusing tension on the Korean peninsula.

The book contains several minor typographical and factual errors. "Hysho Iwashima" (p. 14) should be spelled Hisao Iwashima; "Hua Sun-ping" (p. 14) should be changed to Sun Ping-hųa; "Eisaki Sato" (p. 23) should be spelled Eisaku Sato. Also, the quotation of a passage from the 1977 Japanese Defense White Paper should read: "the strategic arsenal of the

Soviet Union is *now*..." instead of "is not..." (p. 6). And the island of Okinawa reverted to Japan's control not in 1969 (p. 18) but in 1972. Perhaps a more serious weakness of the book can be found in the fact that it does not deal with the two important developments which have been significantly changing the security environment in Northeast Asia: (1) the full-fledged normalization of Sino-American relations; and (2) the growing economic and political ties between Tokyo and Peking.

These minor errors and defects notwithstanding, this is "must" reading for anyone who is interested in international relations in East Asia. Not only is it informative on the changing security environment in Northeast Asia, but it also presents by far the most systematic, articulate, and cogent arguments against the Carter administration's troop withdrawal plan. The suspension of the plan by President Carter in 1979, following a careful reassessment of the military balance on the Korean peninsula, has vindicated in a sense the validity of the thesis expounded by the contributors to this volume.

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Global Inequality: Political and Socioeconomic Perspectives. Edited by D. John Grove. (Boulder, Colo.: Westview Press, 1979. Pp. xiii + 236. $18.75.)

It risks triteness to note that a collection of papers originally presented at a conference customarily suffers from (1) a lack of effective focus among the papers and (2) marked variations among the papers with regard to assumptions, approaches, and quality. This collection of papers (presented at a conference on global inequality, held in June 1977) represents no exception to these almost intrinsic shortcomings. In a two-page preface, the editor touches upon the substantive and unreconciled differences in viewpoints expressed by the contributors and, in refraining from offering either an introductory or summary essay, he wisely resists the temptation, to which editors characteristically surrender, of attempting to paper over these indisguisable inconsistencies. The lack of integration becomes apparent upon a reading of the first two essays. One of the major points made in the introductory essay by Raymond Duvall is that in measuring inequality between social aggregates, it is essential to acquire data on variation within the aggregates (i.e., comparisons based on per capita incomes and the like are faulty in that they tell us nothing about the distribution of wealth within the aggregates). However, in the second essay William Tabb employs the black community within the United States as an otherwise undifferentiated economic unit, despite the availability of much data chronicling increasingly sharp economic divisions between a substantial and increasing black middle class and what William Wilson terms "a black underclass" (*The Declining Significance of Race*, Chicago, 1968).

Given the inherently heterogeneous nature of such anthologies, it would appear reasonable to approach their review much as one might comment on a given issue of a journal. With regard to the latter, one is apt to say that it is an interesting/mediocre/poor issue and then draw attention to the article or articles one found most interesting. On this basis, the collection definitely merits attention for addressing an interesting and important topic. The degree, causes, and potential political consequences of global inequality are self-evidently matters of broad and vital concern. Unfortunately, the literature for years has been dominated by cliches, such as "the have and have-not nations [sic]" and by the most superficial analyses. The present collection is therefore to be applauded for pointing out some of the pitfalls to be avoided and for suggesting some of the directions that more sophisticated analyses might take. Along the way, many of the key questions are raised: How does one measure inequality? What is to be the unit of analysis (e.g., a religious group, a national group, a state's citizenry, a class, an age group, a sexual classification)? How will the selection of the unit influence the result?

Of particular interest was Elise Boulding's treatment of the relationship of inequality to sex and age. Boulding proffers a great deal of trans-societal data documenting the generally disadvantageous economic situation of (a) women and (b) the young and old. But she also most fairly presents data running counter to these generalities, such as the longer life expectancy and lower suicide rate experienced by women in nearly all societies. In another essay, John Grove offers an interesting, self-admittedly exploratory study of the relationship between (a) the level of economic development achieved by a state and (b) the relative distribution of wealth among groups within its confines. In a parallel piece, Baldave Singh also offers a stimulating essay and a great deal of data. At a much more micro-analytical level, Raymond Corrado, who has formerly published a number of enlightening pieces on Welsh nationalism, collaborates here with Bert Rockman in comparing attitudes toward inequality on the part of Welsh and English peoples living within Wales. In a short piece, Bruce Russett offers data linking life expectancy to income. (Incidentally, the reader may be confused by the fact that Russett's graphs on pages 65 and 67 have been inadvertently interchanged.)

As is customary, I should express a few minor reservations. (1) Perhaps because of the nature of the focus, the contributors appear to share an exaggerated view of the consequences of economic disparities. There are numerous cases in which groups have elected to accept a lesser economic status because greater value was placed upon some other standard, such as political independence. (2) Statistics are presented as though they serve as the triggers of social action. But, of course, it is not reality but the perception of reality that has socio-political consequences. Only the essay by Rockman and Corrado deals with perceptions of reality (and it offers the rather unexpected example that Welsh urbanites believe that they get better jobs than do Englishmen living within Wales, even though the opposite is the case). However, the coauthors then forget this key distinction between perception and reality, concluding that a large economic transfusion by London, if feasible, "should have a significant impact in eroding support for Welsh political nationalism" (p. 317). (3) Much of the data throughout the collection are, in any case, of very questionable reliability. In many cases, the data are derived from the UN and/or governments and refer to political systems in which there is seldom adequate information available even on the number of inhabitants, much less on the distribution of economic assets. (4) Finally, the word *nation* is misused throughout when what is intended is a state. While the practice of confusing these key terms unfortunately is the rule among political scientists, it is particularly misleading and dispiriting to encounter the practice in a work often using as units of analysis a broad number of substate and transstate nations, such as, *inter alia,* Armenians, Croats, Englishmen, Flemings, Malays, Maori, Punjabi, and Sinhalese.

WALKER CONNOR

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Israel-Latin American Relations. By Edy Kaufman, Yoram Shapira, and Joel Barromi. (New Brunswick, N.J.: Transaction Books, 1979. Pp. xvii + 256. $19.95.)

This is not an easy book to read or to review. There are "absurds" instead of absurdities (p. 75). There are United Nations "votings" instead of votes (throughout chapter 5). A subheading is called "Significant Notes" when it should be called Significant Votes (p. 153). We read about the UN's "foundation" instead of its founding (p. 188). There is a 31-page chapter with 109 notes and a 56-page one with 207 notes. There are 49 tables and charts, or one for every 5.2245 pages. And to make things really fun for the serious scholar who must shuttle back and forth between the text and the endnotes—except for a mention on the contents page and another at the beginning of each chapter, the name and number of each chapter is to be found nowhere else. In short, in this book, style is an enemy, not a friend of substance.

Nevertheless, readers with the skill and patience to overcome these boobytraps will find an excellent examination of the relations of the countries from approximately 1947 to 1973. They will find a book that succeeds in its stated aim of evaluating "the attitudes of Latin American decision makers toward Israel . . . [by studying] patterns of behavior, the grouping of nations according to levels of support for Israel, and an analysis of the influence of different variables on the policymaking process. . ." (p. xi).

This is done via six chapters: "External Setting," "Internal Setting," "Israel's Instruments of Foreign Policy," "Latin American Decision Makers and Their Psychological Environment," "Latin American Voting at the UN General Assembly" (perhaps the best chapter in the book though not the best written one), and "Diplomatic Bilateral Relations." (Shouldn't that be "Bilateral Diplomatic Relations"?)

Appendices A and C are especially valuable and handy. The one lists significant votes; the other, the exchange of trainees and experts in Israel's technical assistance programs with Latin America. With regard to the latter, it is interesting that while Latin America as a continental grouping has generally been Israel's staunchest supporter, particularly in the early years, it hasn't fared as well as Asia and Africa (see pp. 121–22).

For years, there have been those who have urged Israel to give Latin America a higher diplomatic and economic priority. But to no avail. It has always been put last. If you want to know why and at what (changing) price, read this book.

EDWARD BERNARD GLICK

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A Continent Astray: Europe 1970–1978. By Walter Laqueur. (New York: Oxford University Press, 1979. Pp. vii + 293. $15.00.)

In general, truly bad books do not merit review in a major journal; but I believe this one to be a non-trivial exception. Its subject, author and publisher are prominent; and its intrinsic and representative flaws may be instructive. Walter Laqueur is an eminent writer who holds important positions at major, university-affiliated research centers on both sides of the Atlantic (simultaneously); he is editor of several journals and series of occasional papers; and he has tended to be occupied with the salient issues of each of the past three decades. His audience encompasses large segments of the policy, academic and general intellectual (read "opinion-making") communities in Europe and America. His ostensible subject, "the crisis" of contemporary advanced industrial societies, is one of the significant concerns today. The publisher is a university press long esteemed for both its scholarly and trade titles. This book is presumably intended to straddle that often artificial and useless boundary; and the policy relevance of its subject is obvious. But, after several readings, the only justification for the book I have been able to conjure up is that any and all work by a prominent writer deserves uncritical–indeed, unedited–and swift publication.

Laqueur opens his book with a dual thesis: "The history of [Europe during] these last years is the history of a crisis, mainly political in character, even though it was triggered by an economic recession" (p. 3). While I might quibble with the use of the term "crisis," I fully share the author's judgment that the problems underlying the recent difficulties of advanced industrial societies in general and European countries in particular are essentially political.

But what is the nature of the underlying political malaise in Europe? Laqueur believes it to be drift and disunity: "Europe of the 1970s presents a melancholic picture of fragmentation, internal squabbles, and aimlessness" (p. 6). These would be serious problems indeed, if they could be demonstrated. In order to make such a case, it seems necessary to address questions such as "What is fragmented?" "Who is squabbling–and what is internal?" (That is, is the squabbling internal to Europe, however, it may be defined, or to each of its states?) And, "Who is aimless; and how is such aimlessness manifested?"

Laqueur (with Herbert Block, an economist, whose authorship of the chapter on "Euroeconomics" is noted only in the preface) seeks to demonstrate the cogency of his theme and the validity of its conclusions in a series of synoptic snapshots that hardly comprise either a comprehensive overview of contemporary Europe or a coherent picture of a continent adrift. The book suffers from fundamental flaws at at least three levels–the first and second each fatal, the third at least extremely irritating and perhaps more.

First, the structure does not permit a systematic overview. Brief chapters on "European Politics, 1970–1978," "Eurocommunism," "Eurosocialism," "Euronationalism," the aforementioned "Euroeconomics," and so on do not permit coherent assessments. They are neither the categories of European politics and policy nor analytically useful rubrics. The result is a series of skimpy sketches with overlaps of no particular importance or evident purpose. If Laqueur attempted a sustained essay, he failed. Moreover, by fragmenting the descriptive material and his interpretation of it in such a manner, he has, arguably, projected an image of incoherence onto his subject that may be more an artifact of the organization of the book than of European developments. Further, by subdividing most of his chapters into three- or four-page fragments depicting country-by-country developments, Laqueur reinforced an impression he seeks to convey, but one that is neither substantiated in the book nor, in my judgment, is in accord with reality: that the approaches taken by Western European governments and political parties to common or similar problems differ appreciably in form and effectiveness.

Second, Laqueur's strong opinions lead him to emphasize developments of which he disapproves and on occasion to ignore important facts that are inconvenient. Among the former, the increasing political salience of ethnic and regional movements and the customarily unprogrammatic postures of socialist parties stand out. These elements of recent and current European political life need to be analyzed, explained and put into perspective rather than merely deplored. Thus, with regard to "Euronationalism," there is a near-complete insensitivity to differences between demands for self-determination and partial autonomy in some form (e.g., at p. 163). Among the latter, one glaring omission stands out: the author's extremely favorable picture of democracy in Germany (pp. 69–73) is presented without mention of the excesses to which the present government in particular and the regime in general have gone to deal with terrorism, undemocratic views among public employees and lesser threats. His opinions, rather than a careful, comparative consideration of evidence, also seem to account for his jaundiced view of the French regime's performance in economic and other terms. Neither the opinions of most informed observers nor a comparative analysis of the data would support such views. If this was intended to be a book of opinion, aiming to convince those not already sharing Laqueur's views, it must be judged weak because his predilections are too obvious and crudely presented to influence any but the like-minded.

Third, these problems are compounded by an inordinate number of frequently very serious factual errors and/or what may well be a record number of important editing lapses. In a short review only a few can be noted, as illustrations, but the book abounds with similar or worse mistakes. For instance, there are incomplete sentences; references (p. 52) to the "severe recession of 1975–75" (which may have been severe indeed, but, if the author's dates for its duration are accepted, it must have surely been the shortest on record), curiously "balanced" elsewhere by a reference to the "current mini-pause in the world economy"; a reference in one place (p. 43) to "Greek-Turkish fraternity," but elsewhere a recognition of the long-term, serious hostility between these two countries; the misspelling of a German newspaper's title; a badly butchered, pompous and gratuitous French phrase (why use it if you don't know it or sufficient French grammar to realize that it is wrong?); and, incredibly, a reference to a GNP of $14 trillion in Scotland! Such mistakes seem indicative more of a failure to think through and to edit than of mechanical lapses of the sort encountered in the production of most books. They exacerbate the poor organization and opinionated tone of the book.

In sum, Laqueur has depicted normal political life in contemporary democratic Europe and found it unsatisfactory for coping with a set of radically altered circumstances. He does not like what he sees. But, instead of measuring performance in terms of either the tasks with which the politics in question must try to cope, or some other explicit standard, he has condemned political actors and entire societies for failing to measure up to some implicit—and not really discernible—expectations he holds. The very inchoateness that bothers Laqueur so much could be viewed as a laudable openness and effort to muddle through very difficult times, of a valued pluralism and political competition. It is not at all clear that Western European states are performing poorly; what is clear, from Laqueur's book, is that he doesn't like the prevalent style. The reaction with which this reader is left is, "so what?"

MARTIN O. HEISLER

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Diplomacy: New Approaches in History, Theory, and Policy. Edited by Paul Gordon Lauren. (New York: Free Press, 1979. Pp. xvi + 286. $25.00.)

Diplomacy demonstrates how diplomatic historians and political scientists can learn from each other and enrich their respective endeavors. Every scholar of diplomacy and every policy maker seeking historical analogues should examine the articles written especially for this volume by 10 highly respected scholars. Each employs an interdisciplinary approach, assesses the value of his particular approach and presents specific instances where the past is studied in new ways using theory or theories are examined more critically in terms of historical evidence, e.g., the War of 1914, the Cuban Missile Crisis, the Helsinki Accords, SALT, the Franco-Prussian War.

Historian Gordon A. Craig shows the relevance of certain old works to contemporary transactions among nations. Political scientist Alexander L. George offers procedures for developing policy-relevant theory grounded in systematic examination of historical experience. Historian Marvin Small shows where diplomatic history and quantitative international politics intersect, using a study of the 1870s to illustrate different approaches and their limitations. Contributions of the quantifiers include content analysis, e.g., public opinion, diplomatic documents, leaders' views, events data, and simply the vast amount of data that has been gathered.

Six scholars explore specific theories highlighting the benefits and limits of cross fertilization. Political scientist Ole Holsti, who is currently serving as president of the interdisciplinary International Studies Association, draws upon diplomatic documents and psychology in an interdisciplinary inquiry of time pressure in 1914 crisis decision making which lends support to the hypothesis that one reaction to decisional stress is hypervigilance. Historian Samuel R. Williamson, Jr., suggests that more complete and less distorted historical accounts will emerge if historians pay attention to organizational factors. Drawing upon vignettes of the Austro-Hungarian monarchy in 1914 (e.g., the impact of harvest leaves), he suggests that political scientists working in organizational processes can serve "as a prick to the historian's mindset" (p. 141).

Tracing escalation theory since the Korean War, Richard Smoke observes that its development until recently has been mainly by theoreticians making only rare use of real diplomacy and military past. In his "sketchy" comparison of incidents in the Franco-Prussian War and the Cuban missile crisis, he finds some similarities in the roles of Bismarck and McNamara concerning escalation. Historian Paul Lauren employs examples spanning many eras to show that theories of deterrence and coercion may help historians explain why some cases of bargaining with threats of force succeed and others have failed. In systems theories, political scientist Robert Jervis contends, we find patterns, e.g., interconnections, that stay the same as personalities and domestic politics change. Finally, historian Roger V. Dingman's effort to understand alliances leads him to call for their conceptualization and alternative analytical approaches. He includes cybernetics, organizational and bureaucratic approaches in a brief study of the decision to insert the clause in the U.S.-Japanese Alliance of 1951 regarding the use of U.S. troops in internal affairs.

Three other characteristics of this volume deserve attention. The style is almost universally interesting, free of jargon, and often interspersed with humorous critiques. Small, for example, cites an author who observes that it is dangerous to assume much similarity between the Franco-Spanish War of 1823 and the Football War of 1969 in Central America even though both had an estimated 1000 battle deaths. Samuel F. Wells, Jr.'s final chapter includes a challenging list of issues and cases for historians and political scientists to explore in order to assist policy makers. Finally, an especially helpful index cross-references terminology from the different disciplines.

JOAN KRUEGER WADLOW

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Human Migration: Patterns and Policies. Edited by William H. McNeill and Ruth S. Adams. (Bloomington: Indiana University Press, 1978. Pp. xviii + 442. $22.50.)

This volume brings together 19 papers presented at a conference on human migration organized in 1976 in honor of the United States' bicentennial. The senior editor, William H. McNeill, states that "papers were designed to cluster around three foci: (1) a sampling of historical patterns of migration against which to measure more recent manifestations of humanity's penchant for moving about; (2) a survey of migration movements in major segments of the earth since World War II; (3) a weighing of public policies towards migration in the light of the best conceptualization of the process we could bring to bear" (p. xv). The contributors, drawn from the United States, Europe, and Australia, represent several disciplines in the humanities and the social sciences.

The three historical chapters, devoted to Europe and Asia, illustrate the importance of migration as a determinant as well as a result of social change in these regions over the past several millenia. James Lee shows how migration built China as a geographic and multiethnic entity. McNeill's chapter adds the theme of the close and varied relationship between incidence of infectious diseases and migration—familiar perhaps to some readers from his earlier work (e.g., *Plagues and Peoples,* Doubleday, 1976). Another theme permeating all three chapters, perhaps surprisingly to some readers, concerns the considerable mobility of agricultural populations.

In terms of the structure imposed on the book by the editors—but not of the individual chapters' intrinsic value, for most are excellent—the next two parts are the weakest. Introduced as surveys of contemporary population movements in Europe and the Americas, and in Asia and Africa, respectively, they exhibit little system and consistency with respect to the selection of regions or countries covered, the types of migratory movements analyzed, the recency and comprehensiveness of quantitative data presented, and the extensiveness of references to other relevant literature. (Quite in contrast, for example, to *The Politics of Migration Policies,* edited by Daniel Kubat et al., New York: Center for Migration Studies, 1979, which succeeds in being comparative despite reliance on a multiplicity of contributors.) For example, the section on Europe and the Americas contains chapters on Europe, the United States, the Caribbean countries, and Argentina: not only are Canada and most of Latin America not represented, but also the chapter on the U.S. discusses neither its legal nor its illegal immigration, and data on internal migration are offered only as they are relevant to the central argument—a brief, speculative, and very interesting note suggesting a reformulation of Turner's frontier thesis, but hardly a survey of contemporary U.S. migration. Bennigsen's and Wimbush's chapter on Soviet European migration to Soviet Central Asia, which neglects any comparisons with other parts of the Soviet Union and thus fails to establish which of the phenomena discussed are unique to Soviet Central Asia and which are common to the country as a whole, would have benefited greatly from incorporation of Brian Silver's research results (published in this *Review* and in other American social science journals); the absence of any reference to his work is glaring.

Certain inconsistencies concerning terminology and format could have been easily removed by more stringent editing. For example, the term "chain migration" is used in its generally accepted meaning by all but one of the authors discussing the particular phenomenon (pp. 53, 80, 353, but not p. 35), while that author (Lee) uses the term (p. 29) to signal a different process, which is referred to as "step" or "step by step" migration by Charles Tilly and Carl Solberg (pp. 56, 158). And while three chapters contain a separate, alphabetic listing of bibliographic references, in the other chapters the references are included with the substantive notes—a style that offers much less convenience to the reader.

The eight chapters in the final part, on "Problems of Theory and Practice," raise a wealth of interesting theoretical and policy issues. Aristide Zolberg argues eloquently that in most attempts to explain *international* migration patterns, models based solely on axioms of individual rational choice will be useless or worse: such models must include policy variables, and he successfully develops a framework for such an analysis. (That this century has brought increasingly stringent limitations on people's movements across borders is an empirical fact repeatedly noted throughout the book.) But two of the most interesting theoretical contributions are contained in the book's earlier parts on migration patterns: the theoretical framework offered by T. G. McGee for the analysis of rural-urban mobility in South and Southeast Asia, and Hans-Joachim Hoffmann-Nowotny's brief synopsis of his important theoretical work on the sociology of migration, which regrettably is accessible in its entirety only in German. That chapter is also an excellent contribution to policy analysis, as is Gerald Rosberg's chapter, in part 4, on the "crisis" of illegal immigration to the U.S.

On balance, because of its many outstanding individual contributions, and despite various editorial shortcomings, *Human Migration* certainly deserves a place on the shelves of scholars interested in migration and, more generally, in social change, and I expect that it will find substantial use also in graduate teaching.

ROSEMARIE ROGERS

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India: A Rising Middle Power. Edited by John W. Mellor. (Boulder, Colo.: Westview Press, 1979. Pp. xv + 374. $20.00.)

The first observation which must be made about this volume is that it is the product of a series of meetings on United States aid policy toward India held in 1976–1977. It therefore reflects the first flush of optimism within the American fraternity of India experts occasioned by Jimmy Carter's campaign and election to the presidency. During the long lead time preceding publication, the context has altered sharply. In a world of skyrocketing oil prices, dollar decline, double-digit inflation and unemployment in the Western world, not to mention the manifest weakness of the United States in the Iranian and Afghan crises, it will be hard for the average citizen in 1980 to take seriously the authors' proposals for giving billions of additional dollars in aid to any country, or to be moved by the rhetoric of "North-South Reform." On the Indian side of the ledger, the poor performance and collapse of the Janata government has belied the projection of political stability which most of the authors made.

The conference participants were for the most part a familiar group of what one of them calls the "international aid community": American officials, academics and foundation-wallas, sprinkled with a handful of resident Indians, who share a whole world of liberal values and assumptions about United States foreign policy, about Indian politics and about international economics. For instance, those who mention Indira Gandhi's Emergency rule of 1975–1977 set it within quotations and condemn it. The only refreshingly discordant note is provided by the wrongheaded but provocative Baldev Raj Nayar who manifests a "Third World" suspicion of American motives which the rest do not question. The conference commentators' remarks are helpfully printed after each paper, but are only marginally critical.

The title of the volume is a misnomer. It is only incidentally about India's role, whether rising or not, in international relations and might better have been "The Revival of American Aid to India."

With the drastic change of circumstances since 1977, one misses a general framework in the book for analysis and evaluation of aid policy, be it Realist, Liberal or Marxist, to give the papers more lasting significance and to prevent them from becoming rapidly obsolescent in the 1980s. The editor promises in the preface a "re-examination of American foreign assistance to India," but none of the contributors really tries to answer the hard, basic question raised by Mellor in both the introduction and the conclusion: "Does India's development matter to the United States?" Humanitarian sentiment aside, is it in our national interest? And from the viewpoint of "dependency theory," should India get entangled again in dependence on foreign assistance?

Much of the content of the pieces by John P. Lewis, Hannan Ezekiel and Franklin Long, as well as one of Mellor's, will be of interest mostly to international economists because of their technical nature. For the political scientist, Myron Weiner's, Nayar's and Philip Oldenburg's analyses are more pertinent. George Quester's neat piece of policy analysis on India and nuclear nonproliferation is unfortunately left outside the main thrust of the volume.

Weiner, in his usual functionalist mode of analysis ("managing conflict"), discusses the political impact of American assistance, chiefly in terms of the objectives of India's governing elite during the generation of Congress party rule. As a resource for them, he concludes, it was unreliable, "never without cost or risk to the recipient" which became a political liability outweighing its benefits. Therefore, unlike Mellor and Lewis, he does not anticipate that a return to large-scale bilateral aid (the only kind he thinks is likely to be forthcoming), will be welcome to Indian policy makers even if politically feasible in the United States.

Mellor and Oldenburg see American interests in India as fourfold: ideological (democracy), global (population limitation), commercial (trade) and power (nuclear nonproliferation), but none of the authors demonstrates convincingly that economic assistance has been or is likely in the future to be an effective means to achieve these sometimes contradictory goals. Consequently, the uncommitted reader may be tempted to opt for the Kissinger-like alternative of benign neglect which Mellor raises (p. 360) but rejects.

Finally, the two papers by Nayar are stimulating in that they offer the political scientists in the audience an explicit framework based upon Hans Morgenthau's. He sees India and the United States in conflict over the former's aspirations for Great Power status which the latter, as a "hegemonic superpower" has deliberately sought to contain or frustrate. Nayar's interpretation of American motives is supported largely with quotations from congressional hearings which any student of legislative process should view with a skeptical eye, but the deterministic, power-maximizing model of international relations which he employs doesn't require evidence of self-conscious motives any more than Marxist economic determinism does. However simplistic these assumptions, Nayar's contribution is at least about "India, a Rising Middle Power" and should have been paid more heed by the rest of the conferees. A last note to the publisher: the lack of an index impedes a reviewer's attempts to compare the different authors' ideas.

THEODORE P. WRIGHT, JR.

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Pollution, Politics, and International Law: Tankers at Sea. By R. Michael M'Gonigle and Mark W. Zacher. (Berkeley: University of California Press, 1979. Pp. xviii + 394. $15.95.)

R. Michael M'Gonigle and Mark W. Zacher have written an extraordinary book, one that is notable for both the range of its erudition and the intricacy of its scholarship. Exploring the global political challenge posed by environmental spoilage, *Pollution, Politics, and International Law* focuses on the complex legal and political processes behind international efforts to control oil pollution. In so doing, it presents not only a far-reaching analysis of a major problem of international law and organization, but also a remarkably coherent and valuable treatment of the effects of oil on the marine environment. The product is a rare synthesis of scientific, political, and legal insights that will be of enduring interest to everyone who has confronted the fact of a shared and endangered biosphere.

Pollution, Politics, and International Law deals consecutively with the international organization context of the issues, the progressive development of an international control regime for the environment, and the dynamic political processes of environmental control. It concludes with a fruitful attempt to extend the lessons that have been learned from the politics of ocean oil pollution control to a wider arena of applicability, thereby illuminating promising new strategies for future environmental regulations. This conclusion is of special value because it ties together an array of diverse technical and public policy concerns into a coherent whole that underscores the primacy of international systemic context and interdependence.

Authors M'Gonigle and Zacher premise their book on the unavoidable tension between the requirements of technological expansion, economic growth, and the rising expectations of wealth and the desirability of a healthy global environment. From this premise, they move forward systematically to seek ways to reconcile the intractable pretensions of sovereignty with a new global regime of enforceable norms and regulations. Such a regime flows from the first international controls in 1954 through current efforts to develop an effective transnational and transgovernmental environmental network. Throughout their analysis, the authors focus especially on one international organization, the Intergovernmental Maritime Consultative Organization (IMCO). A UN agency with a 20-year history in the field of international environment regulation, IMCO offers an ideal focal point for understanding environmental pollution in its international political setting.

If there is any potential shortcoming of *Pollution, Politics, and International Law* worth noting, it is one that could not have been readily foreseen by the authors. I refer to the rapidly accelerating obsolescence of their claim, "The 'bipolar' system has fragmented and the 'Cold War' receded. The nuclear threat has by no means disappeared, but the endless jockeying between the superpowers that dominated the politics of the two decades following the Second World War has achieved a relative stability in recent times. Meanwhile, issues of economic and ecological interdependence have begun to predominate over those of war" (p. 11). Regrettably, the recent events in Southwest Asia, reinforced by the possibly irreversible failure of arms control at every level, portend not only a hardening of cold war dualism but also a heightened prospect of nuclear war and oblivion.

Although the authors may have been a bit too hasty in subordinating the urgency of war and peace to the salience of economic and ecological issues, their study of world order reformist strategies for coping with the environmental challenge is generally outstanding. Well-written, well-documented, and thorough in its execution, *Pollution, Politics, and International law* enlarges and improves upon the best works on the subject currently extant. Bound to be studied and taken seriously by all interested students of international economics and international relations as well as by students of the global environment, this book could contribute importantly to the survival capacities of "Spaceship Earth." Indeed, it should soon become as familiar to scholars as a landmark of superb international legal and political analysis as the names *Torrey Canyon, Amoco Cadiz,* and *Argo Merchant* are known to the world at large as a collective synonym for environmental disaster.

LOUIS RENE BERES

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Limited War Revisited. By Robert E. Osgood. (Boulder, Colo.: Westview Press, 1979. Pp. xiii + 124. $15.00.)

A short but timely piece in today's volatile international political environment, Robert Osgood's book analyzes limited-war concepts and their relation to contemporary U.S. foreign policy. The book, which draws heavily from the pioneering efforts of Kahn and Schelling, emanated from a series of somewhat strident meetings on limited war, the thrust of which was "to reassess the foundations of U.S. foreign policy in a changing international and domestic setting."

Osgood defines limited war, in contrast to a total war of annihilation, as those limited both by ends and means:

> Limited Wars were to be fought for ends far short of the complete subordination of one state's will to another's, using means that involve far less than the total military resources of the belligerents and leave the civilian life and the armed forces of the belligerents largely intact (p. 3).

Eclipsed by the nineteenth-century wars of annihilation, limited-war theory surfaced in the twentieth century in reaction to the world wars. Here the Western perspective utilized a Clausewitzian model to employ force as an instrument of ideological containment. Conversely, the non-Western perspective adopted a Maoist approach and employed guerrilla warfare to promote Third World nationalism in opposition to Western hegemony.

As applied during the Kennedy era, the containment policy of flexible response envisioned three kinds of limited war: domestic insurgencies fought by unconventional means, theater wars between states, and central wars between the superpower blocs, but fought within defined limits of engagement. This may be conceptually neat, but, as Osgood notes, this is a Western definition which served the interests of Western nations at a time when the United States possessed both massive strategic superiority and the will to impose its perspectives on cold-war politics.

Western perspectives also proclaimed the successful end to the cold war, as the Soviet Union retrenched its international profile following the Khrushchev ouster. This paralleled for the United States, first over-commitment in Vietnam, later disillusionment, a loss of will, and rejection by the Third World. The Soviet Union, capitalizing on American weaknesses, massively expanded its influence and capabilities, attaining at least a strategic parity with the U.S. Today, an inhibited America struggles with a psychological dilemma, wondering if, having rejected competition in strategic denial capabilities, the deterrent system will continue to constrain Soviet misbehavior. Osgood suggests there is no concept of limited war in Soviet thought: war, once initiated, is to be won whatever the means.

Thus, the United States, its illusions shattered, stands adrift, lacking friends and policy, forced to rethink once more the distasteful cold-war policies of containment.

Containment policy today is certainly more difficult to construct than in the triumphant America of the 1940s. Osgood views the Korean War as the defining experience in the successful application of limited war policy for the U.S. But Vietnam substantially altered U.S. perceptions and, Osgood implies, may have done irreparable damage to Western interests. Thus

> if the Soviet Union exercises a modicum of discretion and avoids precipitating major crises while preserving the semblance of political legitimacy for its interventions, the U.S. reassertion of the concept of containment may be little more than rhetorical (p. 84).

Has the Soviet Union then blundered in Afghanistan?

The chapters on Vietnam are preeminent, an excellent precis of the war and its pernicious effects. The remaining chapters on the future of U.S. foreign policy include Osgood's proposal for a revised containment policy, provided that

> the United States . . . either clearly devalue the nature and scope of its security interests to fit a drastic qualification of the original premises of containment, or else . . . launch a major effort to attain war forces capable of supporting the more demanding premises that still prevail (p. 85).

It is not to Osgood's discredit that several points have been overtaken by recent world events, for he elsewhere was prophetic. Similarly, one need not quarrel over details (was Schlesinger's 1974 strategic nuclear weapons policy really the "first post-Vietnam reactivation of limited war strategy," or was it Laird's mobilization policy?). The only serious weakness is the lack of conceptual clarity in the earlier chapters, important issues imperfectly merged into historical example, itself a reflection of the current confusion in U.S. foreign and defense policies. Notwithstanding, *Limited War Revisited* reopens the academic debate on containment and illuminates the issues which will underlie foreign policies in the 1980s.

ABBOTT A. BRAYTON

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Critical Perspectives on Imperialism and Social Class in the Third World. By James Petras. (New York: Monthly Review Press, 1979. Pp. 314. $15.00.)

The 11 essays in this volume (5 co-authored) both outline an approach to Third World studies and show that theory in action, applied to industrialization, agricultural transformation, class consciousness and revolution. While Latin America provides many of the examples—understandable given Petras' long association with that region—this is not another instance of an area specialist overreaching himself. Petras is well informed on other regions and his central concern lies precisely with what is and what is not generalizable in the Third World.

In common, Third World societies share their (greater or lesser) integration into world capitalism; differences lie in the ways this integration is achieved and resisted. For Petras, therefore, the most universal category for Third World studies is not "development" or even "dependency," but "capital accumulation and exploitation" (p. 183). Petras is critical, however, of world systems theorists (Emmanuel, Amin, Wallerstein) whom he sees stopping there, with an homogenized view of peripheral societies acting out the roles assigned them in the world economy. Petras would shift the locus to a point well within the social structure of individual nations, understanding of course that more than local actors operate there.

For Petras and his collaborators the key question becomes: how do class alliances and conflicts mediate the pressures for accumulation emanating from the metropoli? Here imperialist forces become carnate in political coalitions. Here analysis gets down to the specifics that can inform revolutionary praxis, one of Petras' criteria for intellectual work.

This, then, is a highly political approach. Beyond the emphasis given class conflict, there is close attention to the state, that "crucial instrumentality" for "converting class alliances into development strategies" (p. 48). In addition, one chapter is devoted to state capitalism, a recurring Third World formation in which technocrats and officers—strata not usefully subsumed under vintage Marxist categories, Petras admits—pursue capital accumulation through an expanded state. Finally, Petras insists on a state-focused understanding of imperialism, parting company with those mesmerized by the multinationals. The imperial state, he argues, remains indispensable; it creates and maintains the conditions necessary for corporations and banks to operate abroad.

Is this a useful framework with which to approach Third World realities? I believe so. Is it the outline of "a new approach," a "new Marxist social science," as Petras claims (pp. 10–11)? Certainly it will prove new to many U.S. academics, for whom Marxist analysis is synonymous with ponderous abstractions that leave their empirical appetite unsatisfied. Petras' orientation does not strike me, however, as fundamentally different from that of several Latin American writers, particularly some of those identified with "dependency"—an approach Petras claims "fails to take account of the different social formations within the periphery" (p. 275). One would be hard pressed to argue that Fernando H. Cardoso, to name but one, fails in this regard. In fact, doesn't Cardoso share Petras' suspicion of categories "ripped from societal contexts" and his rejection of "stagnationist theories of capitalist development" (pp. 11, 183)?

Petras puts considerable distance between himself and others writing in a similar vein. When not dismissing their work, he is "demystifying" it (p. 275). Perhaps fine points deserve critical attention. The net effect, however, is a reactive stance that tends to divert attention from the constructive task Petras has set himself. This is a criticism hard to document. It may boil down, in fact, to a question of style. Let me conclude, then, with two criticisms more substantive in nature, each dealing with the alternative to the exploitative pattern of capitalist development Petras has described. Both are important questions, I feel, to which this book offers only rushed or uncritical answers. Neither of them, however, calls into question the basic structure of Petras' analysis, which I find persuasive.

Petras acknowledges that the socialist alternative to the pattern he has described is often "nondemocratic" (p. 294). Party rule has meant worker and peasant rule only to a limited extent. No analysis is offered on this point, however, which Petras sees as separate from his present concerns. But is it? The undemocratic nature of socialist regimes in the Third World may derive from the fact that these societies *also* must accumulate capital at a pace that requires exploitation. Surplus may be extracted through political means under socialism which, while obviously different, can be as coercive as the market and political mechanisms used under capitalism. Petras' unwillingness to deal with this question casts a shadow, I feel, over his tight association of exploitation with forms of accumulation which only accompany capitalist domination. Something significant seems papered over by such phrases as: "The modernization problem is an imperial project. . ." (p. 276).

In a fascinating chapter on the ambivalence which committed intellectuals and revolutionary leaders feel for each other, Petras dismisses as "opportunistic" the intellectuals' demand for "freedom from the constraints of class and party" (p. 202). This judgment closely follows a negative portrayal of Cuban intellectuals since Castro came to power—a portrayal that seems to tar them all with Padilla's brush. But if party rule need not mean working-class rule—a divergence that Petras admits applies to Cuba (p. 293)—then the "constraints of party" hardly equal the "constraints of class." Isn't some autonomy from party necessary, then, if intellectuals are to effectively serve the workers? (Particularly in the late sixties and early seventies, the Cuban party made cultural arbiters out of loyal but unimaginative and untalented hacks.) The question of autonomy can't be settled with a simple attribution of opportunism to the intellectuals, neither in Cuba nor generally.

ELDON KENWORTHY

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The Middle East in China's Foreign Policy 1949–1977. By Yitzhak Shichor. (New York: Cambridge University Press, 1979. Pp. xiii + 268. $25.00.)

What can be learned from case studies, some people ask? Don't they inevitably fail to distinguish between the particular features of the isolated phenomenon they investigate and deeper substantive patterns of more general significance? Not this case study. Yitzhak Shichor of the Hebrew University of Jerusalem has written a book on Chinese policy toward the Middle East which, to my mind, is an excellent example of the kind of intellectual contribution that a well-conceived case study can make. The results are both a careful interpretation of the specifics of China's relations with the countries of the Middle East and important insights into the broader role of China in the global system.

Focusing on a subject which has become one of considerable topical interest since the Khomeini revolution in Iran and the Soviet intervention in Afghanistan, Shichor describes and interprets China's relationship with the Middle East from before Liberation in 1949 to after Mao's death in 1976. He draws upon a wealth of Chinese language materials in addition to sources in Arabic and English, and he constructs an analysis built around several central themes.

Shichor shows how systematic and integrated Chinese foreign policy has been since 1949. Chinese policy toward the Middle East, he argues, flows from Beijing's analysis of the global system. Since even before the Chinese Communist party came to power, CCP leaders have been well aware of the strategic importance of the area, but they have been unable to influence events there. He notes, "Today, despite the most extensive Chinese network of foreign relations, China's real influence in and on the Middle East is probably as small as it was in the mid-1950s" (p. 6). The principal reason is that the Middle East has been the site of competition and confrontation among the major world powers, and China has not had sufficient economic and military capabilities to compete in such a contest. China's consistent objective, Shichor finds, has been to prevent the domination of the Middle East by a hostile major power. In the Chinese view, imperialism and foreign intervention have continually been the source of Middle Eastern problems, and China's answer has been to oppose solutions imposed by the superpowers and to urge that Middle East countries resolve their problems by themselves. Unlike the superpowers, Shichor argues, "China never showed any interest in territorial expansion or in building overseas bases" (p. 7). Moreover, Chinese policy has focused principally on the international politics of the region rather than on efforts to advance communist revolutions. "Chinese subversion in the Middle East has never been a serious threat," Shichor concludes, "Since its foundation, China's main concern in the Middle East has always been with imperialism, social-imperialism, or both" (p. 8).

The body of the book is a chronological treatment of the evolution of PRC policy toward the Middle East which is organized around the major periods in China's global policy. Analysts disagree about what particular dates are the most appropriate cutoff points in periodizing the history of PRC foreign policy, but Shichor's analysis confirms the general interpretation that Chinese policy is derived from an analysis of the global system as seen in terms of dialectical materialism and contradictions theory. The main objective of that analysis is to determine which are the main world contradictions and, on that basis, to identify enemies and friends within the global arena. Chinese policy has generally been designed to build a broad web of relationships among states, political parties, and mass organizations for the purpose of opposing a common, international enemy. A key dimension of this analysis and policy-formulation process is to distinguish principal enemies from those of secondary importance, and to construct policies which will pit potential enemies against each other.

Tracing the evolution of China's Middle East policy since 1949, Shichor describes how it has changed in response to changing analyses of the principal world enemy. Initially, the PRC "leaned to one side," allying with the Soviet Union against American imperialism in the global context of cold war. Then, during the late 1950s and early 1960s, differences between the two major socialist powers led to the formulation in Beijing of a policy designed to confront both the United States and the Soviet Union. Finally, in the period since 1971, China has reversed the pattern of the initial period and now seeks to cooperate with the U.S. against the USSR. Shichor shows how policy toward the Middle East in each of the major periods was essentially a reflection of China's global position based on this analysis of enemies and friends.

Shichor's investigation concludes with the year 1977, but his analysis provides us, I think, with a useful framework for forecasting future Chinese policies. For example, at the writing of this review in early 1980, events in Iran and Afghanistan seem to have precipitated an important shift in relations among the major world powers. "The Soviet invasion of Afghanistan has drastically altered the world situation," asserts the *Beijing Review* of January 28, 1980. The strategic triangle relationship among the U.S., the USSR, and the PRC, which has shaped global politics since 1971, seems to have been replaced by a tacit Sino-American alliance against Soviet expansionism.

Shichor's analysis would predict that such a significant change in global policies would be reflected in the specifics of China's relations with the individual Middle Eastern countries. He shows that previously Beijing called upon the countries of the Middle East to solve their problems by themselves, and that China took pains to avoid association with the two superpowers, which it denounced as imperialist and social-imperialist. Now, however, as a result of the new global alignments, China might be expected to establish a new pattern of more direct involvement in Middle East affairs linked in cooperation with its former imperialist adversary, the United States.

PETER VAN NESS

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La France à l'ONU: Premiers Rôles et Second Rang. By Marie-Claude Smouts. (Paris: Presses de la Fondation Nationale des Sciences Politiques, 1979. Pp. 392. FF 180.)

Marie-Claude Smouts has written the most thoroughgoing analysis of France's role in the United Nations to date. Most importantly, she has examined the remarkable changes since the late sixties in France's approach to and activities in the international organization. This transformation signals alterations both in the attitudes of the French government and in the character of the United Nations.

The grant of a voluntary financial contribution to the UNO announced by Foreign Minister Maurice Schumann in October, 1971 and the acceptance of a French military contribution to the UN Interim Force in Lebanon in March, 1978 symbolize the remarkable change in France's position in the United Nations since the days of isolation in the late fifties and early sixties. Although France continues to hold the positions of principle enunciated earlier and to abstain from votes concerning its overseas territories and special links, France participates vigorously in most debates and demonstrates a degree of political flexibility quite unlike the days of its "splendid isolation," whether imposed or chosen.

To a substantial degree this change in France's fortune is attributable to the shift from cold-war and colonial issues to developmental and regional issues which at one and the same time limit the degree of unity among the Third World states and cast France in a position among rather than apart from the modern industrialized states. In earlier traditional decolonization issues, France found itself often isolated both from the West and the Third World, and hence deprecated and sought to limit the scope of the institution in which these hostile forces converged. Moreover, as de Gaulle elaborated his vision of France as a revisionist power intent on restoring France to the first rank on the world scene, he more often saw the politics of the UN as restrictive or indifferent to France's efforts than as supportive. Hence, in this regard, as in the area of decolonization, the French adopted in the organization a defensive position characterized by legalisms and abstentions. Smouts is much shrewder than many other commentators in seeing that this position carried over throughout the Gaullist period. Although the U.S. was itself increasingly on the defensive throughout the 1960s, and although the French were tempted to mobilize Third World states around its leadership, France's claim to great-power status, as well as its basic political-economic position, placed severe limits on any drastic shift in its UN stance.

As Smouts demonstrates, not only has the shift of issues altered France's UN stance, so too has the character of the organization itself. More and more the institution follows the static conference machinery model and less and less are important decisions taken within the context of the system. Moreover, the relative position of the Security Council has improved, thus bringing the organization closer to France's views on the prerogatives of the council. At the same time, the inclusion of France in the Lebanese forces reverses a traditional understanding that it is precisely from such operations that the great powers should be excluded. On the contrary, France has always insisted that it is preferably through concerted action of the great powers and international guarantees that conflict resolution, particularly in the Middle East, is to be sought. In effect, all of these shifts mean that the UN is both a less dangerous place to do business and one structured in ways, at least temporarily, more compatible with French understanding.

In a sense, then, all of these improvements in France's position in the UN are negative. What positive position has France asserted in the organization? It is in this area, Smouts argues, that France's stance is most problematical. Although France has played increasingly mediative and brokerage roles which have improved its standing within the organization, the result has been to obscure rather than develop emphases on grand designs and France's premier rank. To the degree that France and, more to the point, political groups within France have no desire that the *patrie* become a greater Holland or Belgium but a restored France, then this role is not likely to command very deep support. Moreover, the fact that France *is* a modern industrial state limits its ability to act as a "third party" in any North-South dialogues. On the other hand, to the extent that the European Community states are able to concert positions within the UNO, France may increasingly emerge as an initiator and spokesman for these common positions. Even this role will have to be shared with Germany and Great Britain and it is not clear that important elements of French society would see this as accomplishing the external revival of French power.

This analysis hence brings us to the conclusion implicit in the subtitle of the book—premier roles and second rank. France has indeed evolved important positions both globally and within Europe, but these are reflections not of putative great-power standing but of her middle- or second-rank status. Whether France is prepared to accept the logic of this position is not yet certain, but Marie-Claude Smouts has masterfully portrayed the problem.

ROBERT S. WOOD

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China-Watch: Toward Sino-American Reconciliation. By Robert G. Sutter. (Baltimore: Johns Hopkins University Press, 1978. Pp. xiii + 155. $10.95.)

This is an unusually penetrating and well-balanced analysis of the evolution of U.S.-China relations from World War II to shortly after the death of Mao Tse-tung. Using the pre-"normalization" tools of the China-watcher (particularly the careful analysis of mass media communications), Robert Sutter portrays Chinese policy as primarily influenced over the long run by balance-of-power considerations. Except for short-term aberrations such as the Cultural Revolution, he sees Peking's perception of vital interests being closely tied to the goal of equilibrium in East Asia. Likewise, American China policy, while periodically subject to ideological aberrations, is seen as ultimately responsive to the requirements imposed by the prevailing strategic balance in Asia.

Using newly released information from the U.S. Archives, Sutter traces the history of U.S.-Chinese Communist relations from the tentative wartime exploration of possible mutual interest through two decades of hostility—encompassing the Korean War, several Taiwan Straits crises, and the Indo-China conflict, to the eventual rapprochement achieved during President Nixon's February 1972 visit ending in the Shanghai Communique. Overtures by Chinese Communist leaders in 1943–44 and again in 1955–57 to secure a more favorable strategic balance in Asia by improving relations with the U.S. were thwarted by Washington's unrealistic attachment to Chiang Kai-shek and fears of monolithic Sino-Soviet expansionism. Sutter sees American policy as shifting from "a pragmatic balancing of national interests within the international state system in the late 1940's to an ideologically based [hence, destabilizing] drive against international communism in the early 1950's" (p. 33), culminating in the U.S.-Taiwan defense treaty of December 1954.

In attempting to deal with American power in Asia, Peking relied initially on the Sino-Soviet Alliance, following Moscow's foreign policy lead—including the "peaceful coexistence" line—until repeated shock and disappointment over the lack of strong or reliable Soviet support for China's policy goals led the Chinese to take an increasingly independent stance. In time, Peking's strategic concerns and determination to prevent the emergence of a permanent "two China's" situation gave rise to a basic shift in which calculations of power and national interest began to triumph over political ideology.

Sutter presents a very readable survey of these developments and a reliable analysis of the factors behind them, often skillfully employing content analysis to unravel important problems not fully explored in previous scholarship. A case in point is his chapter on China's "first overture" to the Nixon administration in late 1968, with its intriguing analysis of divergent opinions in the top Chinese leadership over the possibility of a move toward the U.S.—and the subsequent decision to cancel the proposed resumption of ambassadorial talks in Warsaw. He views this reversal as "a defeat for Chou [En-lai]'s realistic, balance-of-power approach to foreign policy" and "an instance when China's concern for ideological principles and righteousness confounded a pragmatic pursuit of vital foreign-policy interests," placing Peking "in strident opposition to both superpowers at a time when its national security seemed threatened by Soviet power" (p. 82). In the following chapter he goes on to show how the extremely serious Sino-Soviet border clashes in 1969 strengthened the position of Chou and other leaders who favored a pragmatic foreign policy designed to end China's isolation and adjust to the altered strategic balance in Asia.

In response to this basic shift in the power balance, beginning in the late 1960s, Sutter believes that leaders in both Peking and Washington perceived the emergence of a new congruence in their national interests which called for a major realignment of policies toward each other and a "differentiated approach" toward the Soviet Union. The U.S. ended its "previous policy of sustaining the Asian balance against communist expansionism" and developed a new strategy designed to achieve a favorable balance through "the exploitation of Sino-Soviet differences" (p. 110), taking care to reduce the likelihood of renewed confrontations with China over Korea, Indo-China, and Taiwan. Chinese policy, in response to continued Soviet pressures (e.g., the proposed Asian Collective Security System) and the substantially reduced American military threat (e.g., as seen in the Nixon Doctrine and lessened military support for Taiwan), came almost full circle in late 1970 and 1971 to the relatively pragmatic posture of the early 1940s and mid-1950s, regarding China's security as closely related to a reconciliation with the U.S. which alone could provide the leverage to resist Soviet pressures and the threat of Soviet domination in Asia.

Sutter believes that both China and the U.S. have come to view their cooperative relationship as so "strategically beneficial" as to allow them to put aside the numerous issues which have previously divided them. Writing from the perspective of late 1976, following the purge of the "Gang of Four" in China, he sees Mao's successors as likely to continue generally on the course of realism and reconciliation, although we now know that official "normalization" of U.S.-China relations was not to come for two more years.

Notwithstanding the many useful insights and strengths of this survey of Sino-American relations, the reader should be warned that there are occasional gaps in the story. For example, very little is said about developments in the decade 1958–68, which saw renewed crises in the Taiwan Straits and intermittent, but highly important, contacts at Warsaw (as in 1962 and 1965). Nor is any light thrown on the puzzling role of Lin Piao. The study generally does a good job of linking domestic politics and perceptions (or misperceptions) to foreign policies, but at some points, perhaps, it does not give sufficient stress to the internal factors. One wonders, for example, whether the Sino-American reconciliation begun under Nixon could have survived the difficulties and backsliding of the Ford administration without the change of leadership in Peking following Mao's death which allowed a moderating of policies from the increasingly strident ideological orientation of mid-1976. In his emphasis on external policies and the balance-of-power (e.g., comparing the present U.S.-China detente to the Anglo-Japanese Alliance of 1902–22, p. 120), Sutter may also underrate the growing bonds of friendship that economics and education are forging between our peoples—beyond the plane of strategic interests. He has already been proved quite mistaken in his low estimate of the potential growth of non-strategic factors such as trade and cultural contacts. Contrary to his conclusion that "Sino-American cooperation will continue to have a narrow base" of dependence upon "the evolving balance-of-power in Asia" (p. 121), subsequent years have shown dramatic growth and broadening of the interests that led in the early 1970s to our reconciliation.

WILLIAM F. DORRILL

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Transnational Capitalism and National Development: New Perspectives on Dependence. Edited by José J. Villamil. (Atlantic Highlands, N.J.: Humanities Press, 1979. Pp. ix + 325. $37.50.)

This collection of essays represents the efforts of a group of scholars to analyze contemporary manifestations of dependency. The carefully interrelated and analytically sophisticated essays make no claim to elevating the dependency perspective to the status of theory. However, by virtue of their insightful analysis of the transnational capitalist system, and the consequences that the latter has for the internal structuring of the political economy of the periphery, the essays provide a much-needed refinement of the earlier dependency perspective. These essays extend the analysis beyond the rather elementary models of surplus accumulation that characterized the periphery's asymmetrical linkage with the metropolis; beyond the unexamined assumptions concerning the superficiality of the state in development matters; and beyond the limited perspectives that emphasized the static nature of class relations in the periphery.

The dependency perspective has always been rooted in the conception that underdevelopment must be analyzed in a structural and historical context. Yet since the earlier conceptualizations of dependence were formulated by scholars who drew their empirical referents from Latin American reality, many students uncritically assumed that dependency was an idiosyncratic manifestation of Latin America's structured relationships with the capitalist world. By their explicit analysis of the processes through which the periphery is integrated into and transformed by the transnational capitalist system, these essays implicitly call out for the serious student to reassess the cross-national analytical and explanatory utility of the dependency perspective.

The transnationalization thesis, which was originally formulated by Osvaldo Sunkel, is the common theoretical strain that elegantly links the articles. Drawing on Sunkel's formulations, the essays examine the domestic consequences of the most recent changes in the global capitalist system—the evolution and implantation of the transnational organization of techno-industrial oligopoly capitalism. Production is organized on a transnational basis by a limited number of firms that have evolved an intricate relationship between "scientific and applied research and development, and mass production and marketing" (p. 69). However, the transnational capitalist system entails more than simply an enhanced capacity to rationalize the organization of capital and labor, and the accumulation of labor on a global basis; it represents an autarkic socio-cultural and socio-political system which through a process of mutual incorporation and marginalization of class fractions generates a dialectical process of disintegration and reintegration of national societies in the periphery. Local entrepreneurial groups are integrated into the transnational system, while excluded bourgeois sectors simultaneously struggle for the reassertion of national sovereignty.

The 14 essays address the issues of development through their investigations of the multifaceted consequences of transnationalization of capital. In the process a more subtle and refined perspective emerges.

The first four chapters acquaint the unitiated with the basic themes of the dependency perspective and transnational capitalism (Villamil and Sunkel); dependency's evolution as an alternative paradigmatic scheme to the modernization thesis (Valenzuela and Valenzuela); and explore the consequences of transnationalization for the political economy of the periphery (Sunkel and Fuenzalida). Implicit in all the works is an articulate plea for jettisoning specialized disciplinary approaches that abstract concrete situations from their historical and contextual realities. A structuralist-historical framework, which transcends the differentiated disciplinary concerns of the social sciences is called for.

Additional chapters individually explore the structure through which the world capitalist system perpetuates dependency. Individual works examine the structural limitations on the autonomous growth of technological innovation that transnational corporations impose on the developing countries (Fuenzalida); the diffusion of sophisticated methods of mass communication, the implantation of these media and their utilization for generating exotic patterns of consumption (Cruise O'Brien). Luckham advances a framework for analyzing the international political economy of militarism and generates a set of interrelated hypotheses that associate evolving technologically sophisticated military institutions with the progressive dependency of developing countries.

Further chapters apply the transnationalization thesis to specific countries. The authors provide alternative conceptualizations of the role of the state in its interactions with transnational capitalism, and assess the latter's impact on class divisions. Villamil, for example, analyzes the failure of autonomous development through state capitalism in Puerto Rico, and discusses the subsequent role of the state as a major proponent of private foreign ownership of the means of production.

Needless to say, the innovative approach presented in the text needs further elaboration and conceptual precision. Methodological obstacles will invariably confront the most sophisticated researcher. Moreover, it is apparent that the interaction between the transnational corporation and the metropolitan state needs to be confronted directly. However, these are areas that require additional inquiry and contemplation. The dependency perspective, with its emphasis on a holistic approach and structured relations of production, warrants closer scrutiny and serious discussion. The collection of original essays is a major step in this direction.

PEDRO A. CABÁN

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Der Kleinstaat in der Weltpolitik: Aspekte der schweizerischen Aussenbeziehungen im internationalen Vergleich. By Hans Vogel. (Frauenfeld: Verlag Huber, 1979. Pp. 186. SFr. 28.00.)

The topic of Hans Vogel's book is the small state in world politics. He defines "small" in terms of the physical attributes of area and population. Contrary to other authors, Vogel does not use lack of power as part of the definition of a small state. It is rather the main question of the study whether small states tend to be powerless in the international system. For the research design, Vogel has chosen an interesting combination between a large-N approach and a case study. On the one hand, he looks at the universe of all small states in the world, also comparing them often with larger states. On the other hand, Vogel takes an in-depth look at Switzerland as one particular small state.

For the hypotheses, Vogel begins in an eclectic way by making a list of all propositions which he could find in the literature. He has to be applauded for looking both at the literature of political science and international economics. He is right in believing that the two literatures too often are not brought together. As a second step, Vogel tries to organize the various hypotheses in an overall model. Here, he distinguishes two levels of behavior, a structural-deterministic and a strategical-voluntaristic level.

The hypotheses which he categorizes as structural-deterministic postulate a causal chain leading from the scarcity of resources of the small state to the interdependence with the international system, to external dependence and ultimately to a loss of autonomy. The data of the large-N approach show indeed such a trend, but there are many exceptions, especially in the latter part of the causal chain. Vogel concludes that it is not necessarily the fate of a small state to lose its autonomy in the international system. To explain the circumstances under which a small state can escape this fate, Vogel introduces the hypotheses which he categorizes as strategical-voluntaristic. He argues convincingly that Switzerland is a case in point for which these hypotheses apply. Switzerland has chosen a number of strategies which have allowed it to keep a relatively high amount of autonomy. Such strategies are, for example, Switzerland's neutrality, the close collaboration between the public and the private sector in foreign policy decision making and the development of Switzerland as an international financial center. Vogel's insistence on such strategical-voluntaristic elements is a welcome contribution to building theory about the small state.

JÜRG STEINER

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International Trade and Industrial Policies: Government Intervention and an Open World Economy. Edited by Steven J. Warnecke. (New York: Holmes and Heier, 1978. Pp. xi + 245. $33.25.)

International Trade and National Industrial Policy addresses a vital policy issue: the effect of national government intervention in domestic industry on international efforts to maintain an open trade system. Indeed, the recent Gatt negotiations attempted with limited success to address the problem of when government subsidy to industry should be treated as a barrier to trade. The book in fact results from a project intended to evaluate the problems and issues trade negotiators and national policy makers would confront during the Gatt negotiations and the nature of the solutions they might pursue. It is then a collection of essays built around this common problem. The conception of the book is quite clear. It considers, on the one hand, the place of subsidies in domestic policies, focusing on several national experiences. It then attempts to consider the impact of the subsidies on the operation of Gatt rules. The book does not focus on the actual international trade conflicts, but rather on the negotiated rules themselves. The problems in the book result from this choice.

Steven Warnecke, as editor, attempts an admirable and difficult task. His intent was to at once make a useful contribution to an ongoing policy debate in the terms of that debate, while still adding to our analytic understanding of the problems and issues. Judged as to its fulfillment of either task, though, the book is quite uneven. The difficulty is that the book does not systematically link the discussion of the national subsidies to the actual negotiations or to trade conflicts, a failure in large measure a result of the collaborative character of the effort, since many of the authors have taken clear stands about the matter elsewhere.

The volume suggests that these domestic industrial subsidies in themselves encourage bilateral or unilateral political solutions to trade problems. Yet national subsidies in themselves are not trade issues. They are only trade issues when they create disturbance in some other country via the mechanism of trade. Domestic subsidies serve several roles: a response to maintain domestic production in the face of worldwide oversupply, an effort to compensate for a lack of competitiveness, or an attempt to promote greater productivity and efficiency. The question is why have these domestic subsidy issues become pressing trade issues now? The subsidies are not new, and in the most interventionist states such as Japan and France the international trade itself suggests that the degree of intervention has been restrained. In part, it is that the successful reduction of tariff barriers has focused attention on these subtler interferences. It is thus not the expansion of the subsidies, but the changed character of trade problems that has also contributed to the new debate. Countervailing duties have long been a policy in the U.S., but it is the changed trade position that makes them salient. As a wider range of U.S. industries is affected adversely by imports, the coalition supporting free trade has weakened. The institutional adjustments that have facilitated protectionist pressures have been the result not the cause of the problem. Thus the chapters do not really ask which particular policies generate what international problems. As a result, neither the shape of the negotiations nor the pattern of trade development becomes clear.

There are several very interesting chapters and much in the volume is useful; Malmgren offers a very clear definition and discussion of subsidy. Warnecke's consideration of the EEC's efforts to reconcile competing national policies is a useful way of formulating the particular problems of negotiations such as GATT. Namiki's piece on Japan clearly makes the case that the system is not in some simple sense a state-dominated one and that the degree of intervention has in fact dwindled. Yet this chapter sidesteps such vital policies as those for semi-conductors and consumer electronics that have arguably affected the U.S., directly contributing to changed trade patterns. William Diebold's suggestion that we require a system to provide a kind of organized muddling-through of these trade problems, puts the difficulties of the volume clearly—the absence of actual cases that identify substantive issues of conflict. The muddling-through will be in response to the substantive conflicts, and it is those substantive conflicts we must study. The book is a worthwhile conception, a tempting menu; and although it does not entirely fulfill its own charge, it contains some interesting morsels.

JOHN ZYSMAN

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Hydropolitics of the Nile Valley. By John Waterbury. (Syracuse, N.Y.: Syracuse University Press, 1979. Pp. xii + 301. $20.00.)

Let me say immediately that this is an excellent book on a very pertinent topic. Waterbury has gone through the literature very well, including that in Arabic. He has tasted the waters of the Nile and perhaps because of this, he has gone up and down the great river sniffing and looking and listening. Having tackled the mess called Egyptian "statistics," he has become so frustrated as finally to report several sets of figures with the implied suggestion that we accept those that appeal to us most. He has in fact done a tough job that for a very long time needed to be done, and has done it well. Taking my responsibilities as a reviewer very seriously, I looked in all of the crevices and crannies of the book hoping to find errors to pounce upon; however, I didn't come up with much, although Waterbury does have an eccentric way of spelling Atatürk. The book is remarkably free of error. It is a balanced and sober discussion of the river and all the politics that it generates on its long ways to the sea. So much for the review.

Realization of the complexities of sharing the water of the Nile is not a modern phenomenon. But the picture that Waterbury paints of the current and future demands on the river certainly seems very gloomy indeed. There isn't enough water for Egypt, the Sudan and Ethiopia—that is, for their future hopes in increased agriculture, industrialization and hydroelectric power. What the future certainly holds for these countries is feverish attempts at mutual exploitation not only of the river, but also of each other. These attempts are neither always logically conceived nor successfully implemented. (Nowhere have I read a more somber view of the value of the Aswan High Dam than in the low-key factual account that Waterbury offers.)

Sadat may talk grandly of selling water from Lake Nasser to the Saudis, or of diverting some for his new Israeli friends. And most Egyptians have illusory notions of making deserts bloom. But there are deserts in the Sudan too, eager to do some blooming of their own. Indeed the agricultural potential of the Sudan is very substantial. Projects in both Egypt and the Sudan, in the context of the Arab Middle East and Black Africa, soon acquire viable lives of their own, gather momentum and rush into what is often a future of disaster. Yet enlarging populations and accelerating expectations are not to be lessened and the demands upon the river will continue to grow; political and economic pressures on rulers and ruled on all the stages of the river will continue to grow inexorably.

I have thought about the Nile a great deal; I have drifted up and down its muddy waters before Aswan, in and out of flood, but I had never appreciated so fully the many-faceted nature of its problems until going through Waterbury with care. He may not think that he has written a discouraging book, but I was profoundly depressed by the conclusions he has drawn.

This is an absolutely "must" book for all who are interested in the Nile, or in Egypt and the Sudan, or indeed in the world.

CARL LEIDEN

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Reference Sources

Political Handbook of the World: 1979. Edited by Arthur S. Banks. (New York: McGraw-Hill, 1979. Pp. x + 630. $29.95.)

This revised and updated annual survey of world politics examines over 165 countries. Legislative structure, news media, geographical size, population, diplomatic representation, and national political parties and opposition groups are focal points. Intergovernmental organizations are also examined as to their origin, purpose, membership, and activities. New to this edition, the Arab-Israeli peace negotiations, and issues unique to such regions as Southeast Asia, Eastern and Western Europe, and Latin America are detailed. Appendices focus upon a chronology of major international events (1945–1978), membership of the United Nations and its agencies, and statistical data for demographic and economic indices. There is an index.

The Constitutions of the USSR and the Union Republics: Analyses, Texts, Reports. Edited by F. J. M. Feldbrugge. (The Netherlands: Sijthoff and Noordhoff, 1979. Pp. xv + 366. Price not given.)

This volume presents the English and Russian language text of the 1977 Soviet Constitution (adopted in October) in such a way as to enable the reader to see how the adopted constitution differs from its 1936 counterpart and the corresponding provisions in the draft text of June 1977. In addition, the translated preambles and the basic Union Republic 1978 Constitution (with separate republic modifications noted) are presented. Accompanying the constitutions are translations of Leonid Brezhnev's two main speeches which preceded the draft and adopted texts of 1977. The volume is introduced by an analysis of the new Soviet constitution which identifies its major ideological themes, highlights its innovations, and examines the constitution in the context of current Soviet policies and developments.

Five Constitutions. Edited by S. E. Finer. (Atlantic Highlands, N.J.: Humanities Press, 1979. Pp. 349. $28.75.)

This volume contains the full texts of the constitutions of the United States of America, the Federal Republic of Germany, the Fifth French Republic (as well as the Declaration of the Rights of Man and Citizen, 1789, and the Preamble to the Constitution of the Fourth French Republic, 1949), and the two constitutions, of 1936 and 1977, respectively, of the Union of Soviet Socialist Republics. In addition, the editor has systematized those features of British law, custom and convention that make up the unwritten constitution of Britain. The editor has developed two indexes to access the full texts of the constitutions. The first is an analytical index in which the texts of the five constitutions are cross-indexed according to the categories and concepts commonly used by political scientists. The second lists the major features of the constitutions in alphabetical order.

Words and Arms: A Dictionary of Security and Defense Terms. By Wolfram F. Hanrieder and Larry V. Beal. (Boulder, Colo.: Westview Press, 1979. Pp. ix + 265. $24.00.)

This work deals with national security and defense policies. Part 1 defines approximately 800 terms relating to weapons systems, military organization and strategic theories. Brief descriptive essays in part 2 deal in more depth with such policy concepts as "Counterforce Strategies" and "The NATO-Warsaw Pact Military Balance." Part 3 contains statistics, tables and charts summarizing U.S. and Soviet defense capabilities with projections for the mid-1980s.

Britain in Context. By John D. Hey. (New York: St. Martin's Press, 1979. Pp. 189. $22.50.)

This volume is a compilation of social and economic data comparing contemporary Britain with its recent past and the rest of the world, especially but not exclusively with other industrialized democracies. Election data are not included. British social and economic information is considered under three broad topics: "Britain as Part of the World," "Britain as an Economic Aggregate," and "Britain as a Society of Individuals." Brief but analytical textual material guides the reader to multicolored graphic presentations of various styles on pages opposite the texts.

Southeast Asian Affairs 1979. By the Institute of Southeast Asian Studies. (Singapore: Heinemann International Books [Asia] Ltd., 1979. Pp. xi + 363. $37.50.)

This review of developments and trends in Southeast Asia contains articles assessing the politics, economics, and international relations in the region during 1978; articles focused on events in individual nations during 1978; and specialized assessments of topical problems affecting individual states, groups of states, and the region. Sixteen of the 20 articles on individual nations focus on the ASEAN members. The volume also contains chapters on Burma, Kampuchea and Vietnam. The Institute of Southeast Asian studies, established as an autonomous organization by an act of Parliament of the Republic of Singapore in 1968, has published five such yearbooks.

Scholars' Guide to Washington D.C. for East Asian Studies. By Hong N. Kim. (Washington, D.C.: Smithsonian Institution Press, 1979. Pp. xiv + 413. $19.95, cloth; $7.95, paper.)

Scholars' Guide to Washington D.C. for Latin American and Caribbean Studies. By Michael Grow. (Washington, D.C.: Smithsonian Institution Press, 1979. Pp. xii + 346. $19.95, cloth; $7.95, paper.)

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International Encyclopedia of Statistics, 2 vols. Edited by William H. Kruskal and Judith M. Tanur. (New York: Free Press; London: Collier Macmillan, 1978. Pp. xxi + 666 + 1350. $100.00.)

An updated and expanded edition of the *International Encyclopedia of Statistics* first published in 1968, this version contains 75 articles on statistics proper, biographies on 57 eminent figures in the field of statistics, and 42 articles pertaining to areas in social science with a "strong statistical flavor."

Middle East Contemporary Survey, Vol. 2: **1977–1978.** Edited by Colin Legum with Haim Shaked. (New York and London: Holmes and Meier, 1979. Pp. xxxv + 824. $75.00.)

Middle East Contemporary Survey is an annual edition containing analytic and descriptive articles, historical documents and various charts and statistical tables covering political, military and economic developments in the Middle East from late 1977 through 1978. The volume is divided into two parts. The first includes essays focusing primarily on political developments. Several describe and analyze the foreign policies of external actors, consider the attainment and consequences of the Camp David accords, explore some dimensions of the Palestinian issue, and discuss recent developments regarding Middle Eastern oil. The second part of the work is a country-by-country review of the Middle East, covering political and economic developments and including statistical tables. Some of the contributors are Alvin J. Cottrell, Daniel Dishon, Geoffrey Kemp, Colin Legum, Itamar Rabinovich, R. K. Ramazani, Dankwart A. Rustow and Shimon Shamir.

The Papers of Woodrow Wilson, Vols. 27–32. Edited by Arthur Link. (Princeton, N.J.: Princeton University Press, 1978–80.)
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America Votes 13: A Handbook of Contemporary American Election Statistics, Vol. 13. Elections Research Center. Edited by Richard M. Scammon and Alice V. McGillivray. (Washington, D.C.: Congressional Quarterly, Inc., 1979. Pp. iv + 384. $46.50.)

This edition contains 1978 election results, supplemented by past election data to lend an historical perspective. Results included are those for every state primary and general election for governor, Senate, and House of Representatives. Supplementing these election statistics are state maps broken down into counties and congressional districts, a listing of current governors, senators, and representatives, as well as similar data for governors and senators dating back to 1945. Presidential primary preference votes for 1972 and 1976, and state-by-state presidential votes for 1948 to 1976 are included. Population data, by state and county, completes this election reference work.

Congressional Quarterly's Guide to the U.S. Supreme Court. Edited by Elder Witt. (Washington, D.C.: Congressional Quarterly, 1979. Pp. xxii + 1022. $65.00.)

This volume describes and analyzes the history and operations of the U.S. Supreme Court within the framework of American politics and government. Divided into seven thematic parts ranging from "Origins and Development of the Court" to "The Court and the Individual," the work contains a study of the evolution of major doctrines of the Court, an examination of factors which influence the Court, and summaries of all major decisions handed down between 1790 and 1979. Biographical sketches of all justices through John Paul Stevens are also included. Documents relevant to the powers and structure of the Court are presented in an appendix, including acts of Congress invalidated and a glossary of legal terms. There are a subject index and a case index.

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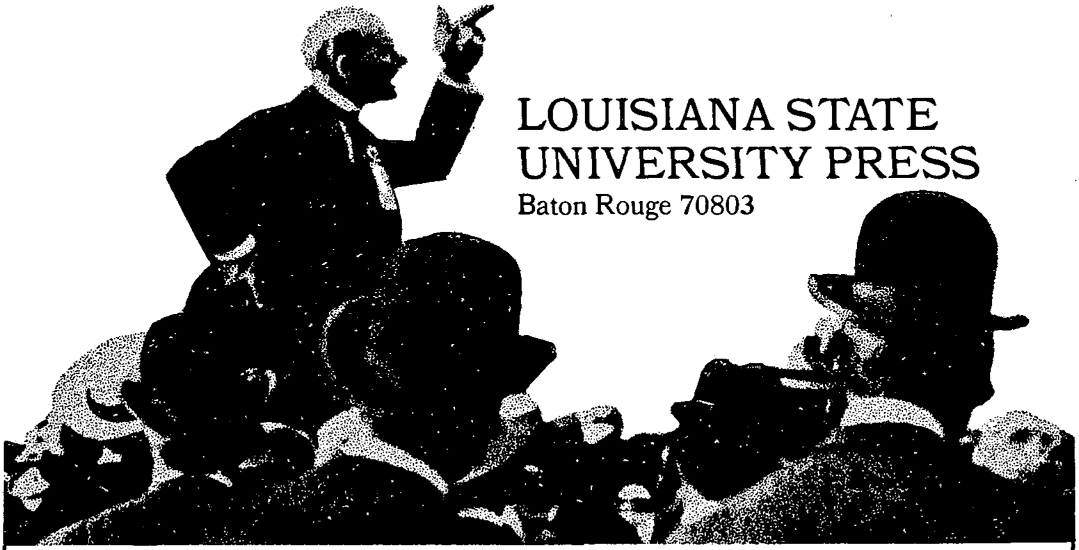

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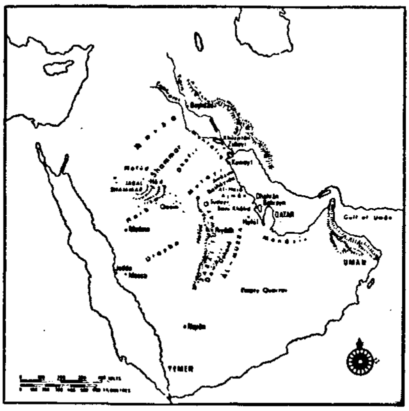

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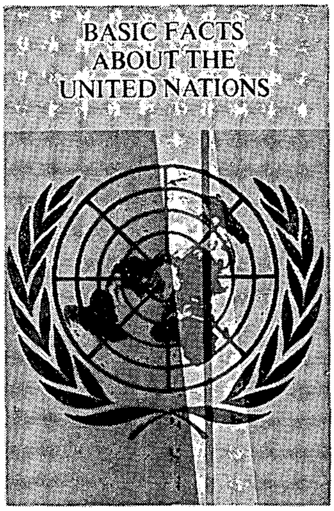

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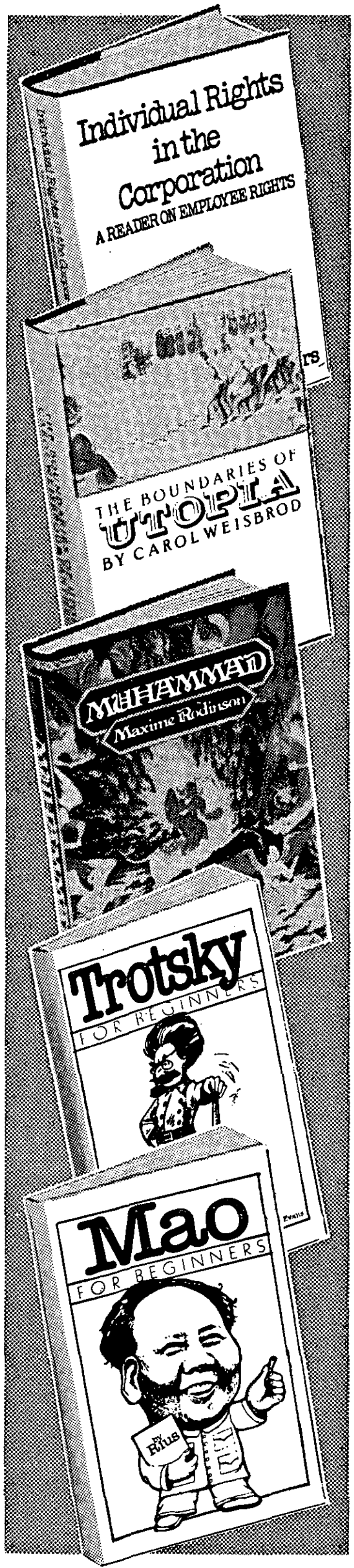

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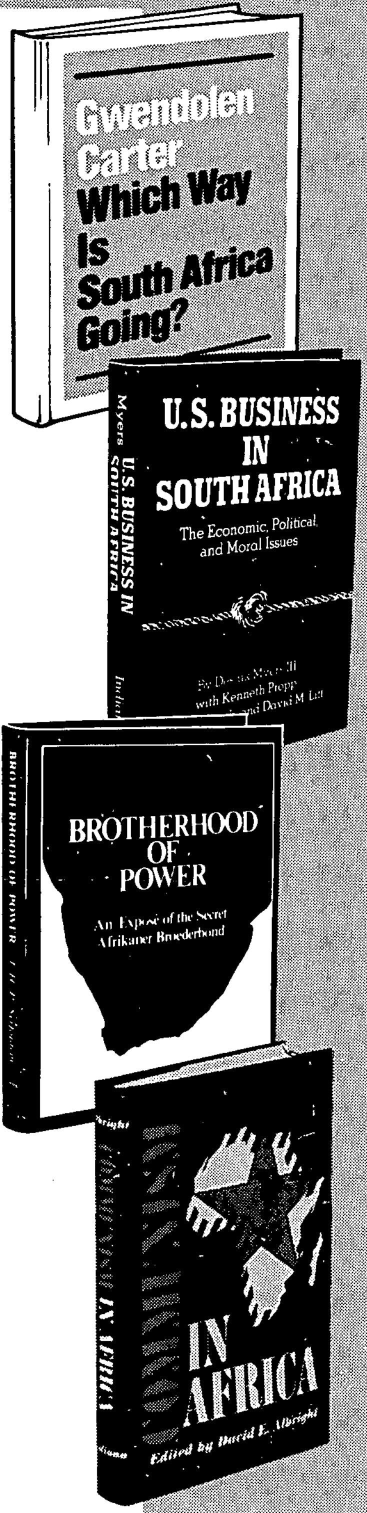

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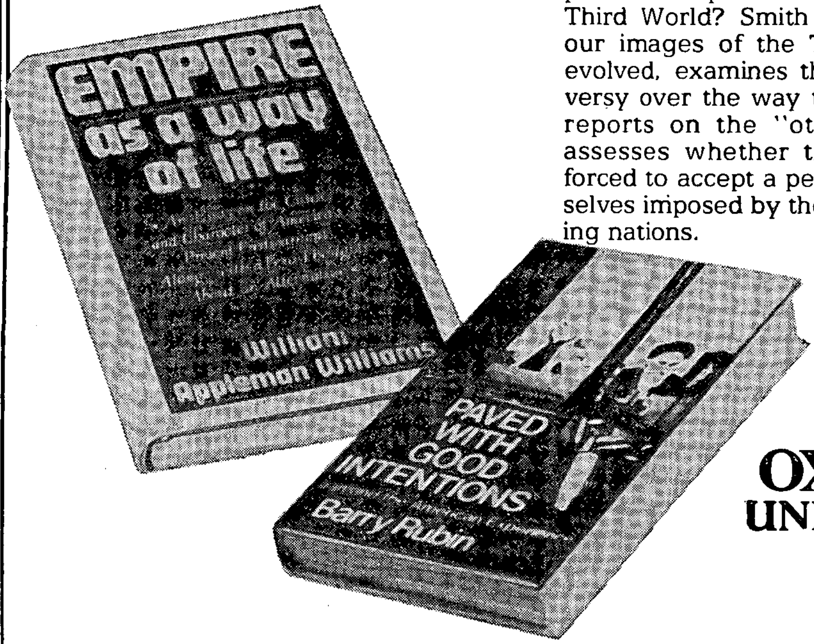

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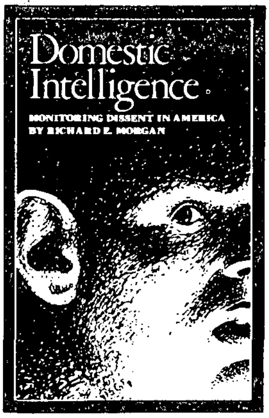

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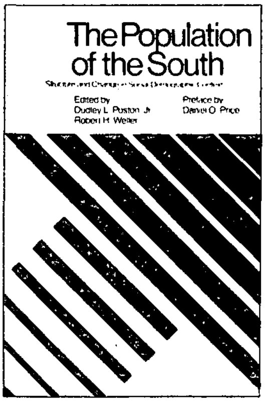

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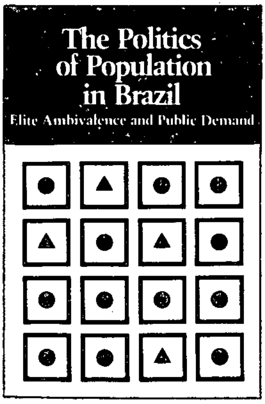
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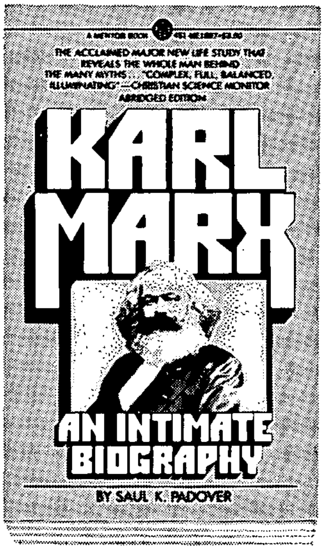
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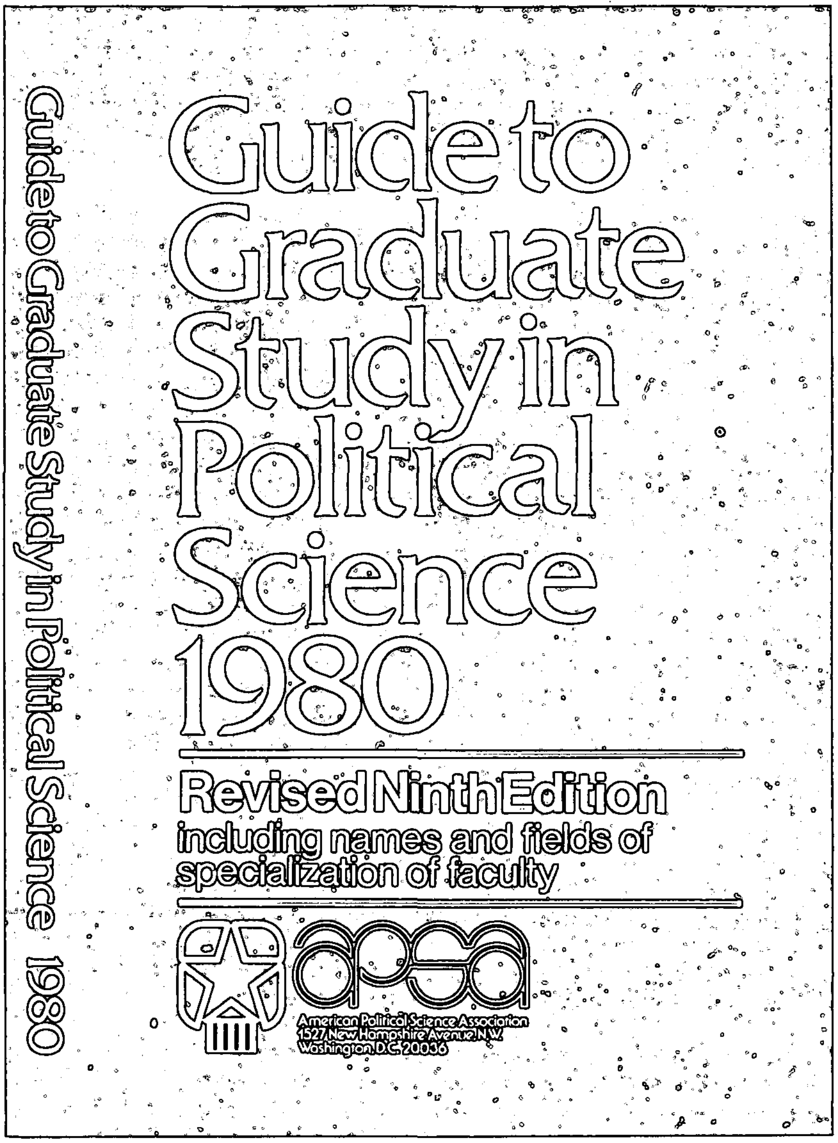

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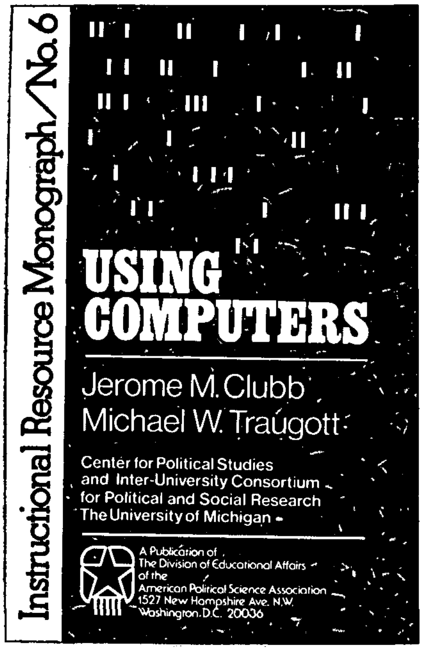

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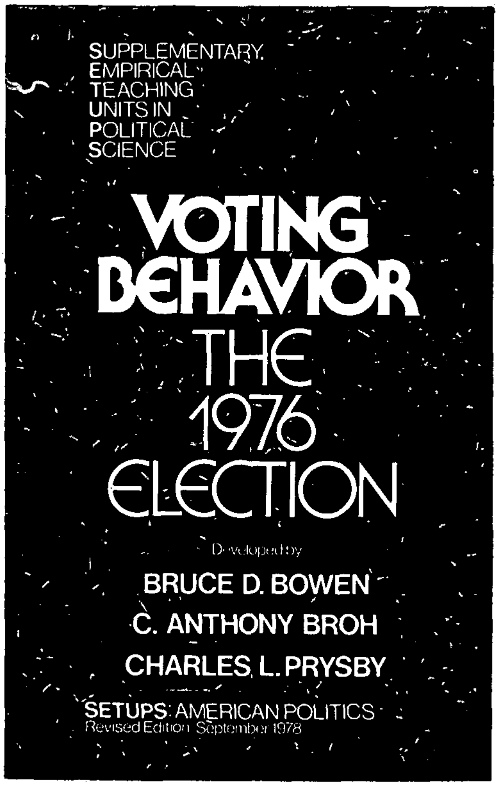

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